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A HUNDRED YEARS OF ASTRONOMY

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by

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TO MY FATHER

PREFACE

In writing this book I have attempted to give an account of the development of astronomy during the last hundred years, which may be useful to the astronomer and at the same time intelligible to the general reader. The account, however, cannot pretend to be complete. During the last half-century astronomy has encroached more and more upon the various departments of mathematical physics, and in such directions its results are often too technical to be included in a book of this sort. Thus certain aspects of astronomy have been only very briefly touched upon, while the work of some of the most eminent investigators has had to be entirely passed over.

Throughout I have made a special point of explaining as far as possible the methods, both practical and theoretical, by which astronomers have arrived at their results. In days gone by the general reader was often satisfied if he were told merely the conclusions of science so that he could feast himself upon the wonder of them without mental effort. But now, I believe, there is an increasing tendency for him to want to take as little as possible for granted and to insist on knowing the why and the wherefore. The universe is indeed mysterious ; but until he can obtain some insight into the methods by which astronomers are beginning to unravel its mysteries, his appreciation of them must be very inadequate. In astronomy, as in all science, it is the manner of the discovery rather than the thing discovered that is so exciting.

The arrangement of the subject matter in the earlier chapters may appear at first sight to be somewhat chaotic. My purpose in adopting this arrangement was to provide the reader as quickly as possible with a general view of the whole situation in order to prepare him for the more detailed discussions in the later chapters.

I wish to express my sincerest thanks, to the Rev. T. E. R. Phillips and Dr. W. H. Steavenson for reading through the MS. and making many valuable suggestions; to Professor H. Dingle for his advice in regard to the section dealing with the interpretation of stellar spectra; to Mr. W. M. Lindley for help with the sections on variable stars; to the Dean of Hereford and Canon A. L. Lilley for reading through the proofs; to Sir Arthur S. Eddington for permission to use, in a slightly modified form the diagram from his Halley Lecture on the Rotation of the Galaxy; to Mr. W. H. White for kindly drawing my two diagrams; and to Miss K. Williams for her help in the preparation of the bibliography.

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INTRODUCTION

BEFORE the invention of the telescope, and while it was still in its infancy, astronomy was concerned almost entirely with the measurements of the positions and motions of the heavenly bodies. Moreover, since the accuracy of such measurement is dependent to a large extent on telescopic power, the heavenly bodies whose motions could be studied were with few exceptions members of the solar system.

The stars, which were far more remote, were known as the "fixed stars." Astronomers charted their positions in the sky relative to one another and determined their relative brightnesses. But they were interested in them mainly in so far as they served as a fixed background to which the motions of the sun, moon and planets could be referred. Hipparchus, the founder of the astronomy of precision, gave us the first catalogue of the positions of the fixed stars; and Ptolemy was the first man to arrange them in order of their brightness. He called the brightest stars "first magnitude stars," and stars that were just visible to the naked eye he called "sixth magnitude stars."

The telescope was first turned on the heavens by Galileo in 1609. The instrument in its original form, with an object glass consisting of a single lens, gave very imperfect definition. About the middle of the eighteenth century Hall and Dollond discovered independently that the performance of the telescope could be enormously improved by making the object glass of two lenses, of two different sorts of glass, placed close together. This was the achromatic refracting telescope in its modern form. Meanwhile, about 1670, quite a different kind of telescope had been invented, a telescope in which the object glass was replaced by a curved and polished metal mirror. The reflecting telescope was brought to perfection by Hadley and reached in his hands a diameter of twelve inches. But it was at the end of the eighteenth century that the reflector made its most rapid advance; for between 1782 and 1789 Sir William Herschel increased its diameter from twelve inches to four feet.

Kepler (1573–1630) discovered that the earth and the planets revolved round the sun in ellipses, and found certain laws which the motions of these bodies obeyed. The most important of them was one which connected the relative periods in which they *revolved round the sun with their relative distances from the sun* So far these laws were quite empirical. Then came Sir Isaac Newton's discovery of the three laws of motion and of the law of gravitation, from which Kepler's laws could be immediately deduced. The law of gravitation stated that every body in the universe attracts every other body in the universe with a force which is proportional to the mass of the two bodies multiplied together and inversely proportional to the square of the distance separating them. It was found that Newton's law explained, at least to a very close approximation, the motions of all the heavenly bodies with which astronomy was concerned, to wit those of the moon, the planets and the satellites of the planets.

The astronomer finds the distance of a heavenly body in exactly the same way as a surveyor finds the distance of a hill. The surveyor observes the hill with a theodolite from the two ends of a measured line and notes the angle through which the hill is apparently displaced against the background of the sky—the change in its compass bearing. The smaller the angular shift, the more distant is the hill. Hipparchus long ago measured the distance of the moon in terms of the diameter of the earth by determining the angular deflection of the moon against the background of the stars, as the earth in its rotation carried him and his observatory from one side of the earth to the other. But the sun was too far away to yield up the secret of its distance to his rough methods. From Kepler's law we can calculate the distance of the sun from the earth as soon as we have measured the distance of the earth from any one of the planets. In 1672 Cassini and Richer made simultaneous observations of the position of Mars in the sky from two distant stations, thereby obtaining the first scientific determination of the sun's distance from us. Many attempts had been made to get the distances of the fixed stars by observing their positions from opposite sides of the earth's *orbit*; but up till a hundred years ago these attempts had all been in vain.

William Herschel was the first man to attain any considerable success in extending astronomical knowledge to the realm of the fixed stars. In 1772 he started with his great telescopes to make exhaustive surveys of the northern heavens. He discovered and catalogued many hundreds of double stars and nebulae, and classified the nebulae in many different types. He was able to show that the stellar system was not of indefinite size and shape, but had a form rather like that of a biscuit, being thin in comparison with its breadth. He found that many of the double stars were twin suns revolving slowly round one another, and were not merely, as had been supposed, the result of coincidence, two stars just happening to lie in line with the earth. Also he found that the sun and solar system were travelling through space among the stars; for ahead of us the stars appeared to be opening out while behind us they appeared to be closing in. Thus Herschel laid the foundations of sidereal astronomy—the study of our stellar system and what lies beyond it—that branch of astronomy which has made such rapid strides during the last hundred years.

CHAPTER I

EARLY ADVANCES, AND THE FIRST MEASUREMENT OF A STAR'S DISTANCE

UP to the middle of the eighteenth century definite astronomical knowledge had been practically confined within the limits of the solar system; but in the latter part of the century Sir William Herschel had begun to extend that knowledge to the system of the stars. In 1822 he who " had looked further into space than ever human being did before him," died, leaving a son who was destined to become if possible even more famous. John Herschel, who was then in his thirty-first year, had been educated at St. John's College, Cambridge, and had come out as Senior Wrangler in 1813. He had been elected a Fellow of the Royal Society at the unusually early age of twenty-one, and awarded its highest honour, the Copley medal, for his mathematical achievements when he was only twenty-nine.

After his father's death John, who had been previously reading for the Bar, decided to repeat his father's survey of the northern skies. In the course of this work he not only added many more double-stars and nebulae to his father's catalogues but amassed a vast collection of measurements that confirmed beyond all doubt his father's discovery that many of these double-stars consisted of suns in mutual revolution round each other. Herschel also took a leading part in the mathematical study of the orbits of these bodies and gave a rigid proof of the truth of the original conjecture that the force which determined such an orbit was that same force of gravitation which held sway in the solar system.

Now there was an obvious criticism that could be brought against the investigations of the Herschels into the shape and structure of the stellar system: the data upon which they were founded were largely lop-sided. A considerable part of the sky never rose above the English horizon, so that all the stars round the south celestial pole to within twenty degrees south of the celestial equator had been entirely left out of consideration. When, therefore, in 1833 Herschel had completed his survey of the northern skies he decided to extend it to the southern hemisphere.

On January 16, 1834—the year in which the main theme of our story opens—Sir John Herschel landed at Cape Town with his family and his twenty-foot telescope. This expedition, which was financed by Herschel himself, was the first purely astronomical expedition on the grand scale and was to lead to results of incalculable value. Never before or since did one astronomer in so short a time reap so rich a harvest. No telescope with anything like the space-penetrating power of the Herschel instruments had ever before been trained upon the southern constellations. Over two thousand double-stars were discovered and measured; about seventeen hundred nebulae were catalogued and classified. The views of the elder Herschel as to the structure of the Milky Way were in general corroborated, though evidence was obtained that the solar system was somewhat eccentrically displaced towards the great star-clouds that formed its southern boundaries. Herschel devised the earliest method for making precise determinations of the brightness of the stars—a problem of paramount importance in more recent years. But perhaps one of the most interesting results of this celestial survey was the discovery of the curious way in which the nebulae were distributed over the heavens: they tended to congregate most thickly in two opposite parts of the sky furthest removed from the girdle of the Milky Way.

The survey took four years to complete and was carried out at an estate called Feldhausen beneath the shadow of Table Mountain. When Herschel sailed again for England his many friends erected there upon the site of the great telescope a granite column which commemorates his visit to this day—the visit of the man who had accomplished the unprecedented task of sweeping the heavens from pole to pole.

On his return to England he was created a baronet, and, retiring from active observing, devoted his time to the discussion of that great mass of material his father and he had collected. He died at the age of seventy-nine at his home, " Collingwood " at Hawkhurst in Kent, and was buried in Westminster Abbey close to the grave of Sir Isaac Newton.

Astronomy of Precision. Meanwhile, in Germany a renewed attack was being made by Bessel, director of the Kŏnigsberg Observatory, on the old problem of stellar distances. We cannot recite here the story of Bessel's early days: how as a struggling clerk in a shipping firm in Bremen he had educated himself and at the age of twenty calculated from some published observations, made long before in 1607, an orbit for Halley's comet. Suffice it to say that as a result he had been induced to throw over his youthful ambition for a seafaring life and take up the duties of an astronomer.

Bessel's work lay in the realm of precisional astronomy, and his life's task was to put the measurement of star positions on a much sounder basis than heretofore. The position of a star read off directly from the telescope requires a great deal of elaborate treatment before it can be entered as a fundamental position in a catalogue. Hipparchus had long ago shown that the celestial pole, to which all star positions are referred, was not fixed but described a large circle in the sky once in every 26,000 years—the earth was rather like a top that wobbles as it nears the end of its spin. Nineteen centuries after Hipparchus it had been found that this circle which the pole describes was not quite smooth but wavy: the pole was slightly nodding up and down in a period of eighteen years as it travelled round the larger circle. The circular motion was called " precession " and the nodding motion " nutation." There was also an effect called " aberration," which we shall explain later on, that caused every star to describe a tiny ellipse in the sky. Bessel devised a systematic scheme by which these three effects could be allowed for, so that observations made at very different epochs could be reduced to a unified system and rendered mutually comparable.

There were other effects too of a more subtle nature that equally required elucidation. When a walking-stick is poked into a fish-pond it appears bent where it enters the water. The reason is that when light passes from one transparent medium to another it is bent out of the straight line. So the light from a star entering our atmosphere is deviated in such a way that the star appears a little higher above the horizon than it really is. The effect is largest at the horizon and disappears at the point overhead. Bessel measured the amount of the deviation at different heights above the horizon and the variations in it due to the barometric pressure and temperature, and published tables by which the effect could be allowed for. Finally he investigated what are known as " instrumental and personal errors." No instrument or observer is ever perfect; but the excellency of the results obtained by either can be brought enormously nearer to perfection by a study of their respective errors. Instrumental errors may be permanent or they may change with time, or again they may vary with changes in temperature, humidity and other conditions: but they can be determined by careful experiment and due allowance can be made for them. The personal error can be treated in the same way, and though the larger part of it—the so-called " personal equation "—can be accurately determined and allowed for, there must always remain an uncertain part, which, as the saying goes, is " only human." If one starts one's stop-watch when the pistol goes one is bound to be slightly, say half a second, late. Half a second is a great deal in timing a race; yet, if one is in practice and takes certain precautions, that half second need never vary by more than, say, a tenth of a second or less either way. Obviously the result will be much nearer to the truth if allowance is made for that half second which is the personal equation.

The routine systematic reduction of all observations in accordance with these carefully worked out methods of Bessel produced an enormous improvement in the results of fundamental astronomy; the measurements were far more reliable, and, what was more, one knew exactly where one was with them. If Hipparchus had laid the foundations of precise astronomy, Bessel had driven them home with a very efficient sledge-hammer.

Armed with these methods Bessel had taken a large series of observations made at Greenwich in the previous century and had extracted from them a highly reliable catalogue giving the positions of some 3,000 stars in the year 1755. By comparing this with the more modern catalogue of Piazzi at Palermo and with his own observations at Königsberg during the years 1821–33 he was enabled among other things to get an accurate determination of the movements of a great many stars during that period. About a hundred years before this Halley had discovered that three of the brightest of the so-called fixed stars were in slow motion across the sky. Since then many more stars with appreciable "proper motions" had been detected, and the elder Herschel had shown that, although part of this motion was real and due to the movements of the stars themselves, part of it was apparent and caused by the movement of the sun and solar system through space. It was now becoming recognized, particularly as a result of Bessel's work, that motion, even though it was scarcely measurable, was the rule rather than the exception among the "fixed stars."

First Measurement of a Star's Distance. A star that early attracted Bessel's attention was a faint object numbered sixty-one in the constellation of the Swan. It had the fastest movement of any star yet known, for an interval of three hundred years would suffice to carry it across the sky over a distance equal to the apparent diameter of the full moon. Now Bessel had come to the conclusion that the stars which showed the most rapid motion in the sky were likely to be on the average our nearest neighbours in space. If one looks out of the window of a stationary train one notices that the men walking along the line pass the window much more quickly than others who are walking faster, or even running, in the field beyond. So Bessel argued that, if he wished to measure the distance of a star, he should confine his attention to those with the largest proper motions as they were those that were most likely to be within reach of our measuring devices. In the past it had been implicitly assumed that the brightest stars were on the average our nearest neighbours, and all attacks on the problem of stellar distances had been confined to such objects.

In December 1838 Bessel published the results of his first year's measurements of 61 Cygni, which revealed for the first time the secret of a star's distance. The earth's half-yearly journey round the sun, which carries it across a distance of 180 million miles, had caused this little star to shift its position in the sky by the apparent width of a shilling held at a distance of six miles—which meant that 61 Cygni was over half a million times as far away as the sun, or at a distance from us of about fifty million million miles.

It so often happens in the history of science that a problem which has for a long time been tackled unsuccessfully, gives way suddenly in two places at once. Thus in January 1839 Henderson, the Astronomer Royal of Scotland, announced the distance he had found for α Centauri, one of the bright stars in the southern hemisphere. The actual measures he had made some years previously while directing the new observatory at the Cape of Good Hope, but it was not until after his return to Scotland in 1834 that he was able to work out the results. Unfortunately, he delayed publishing his discovery until he was able to get some further confirmatory observations made at the Cape, and so forfeited by less than two months the priority of this great achievement to Bessel. α Centauri came out almost twice as close as the little star in the Swan, and is to this day, with one minor exception, our nearest known neighbour among the stars.

Total Eclipse of the Sun. On July 8, 1842, the sun was totally eclipsed over southern and central Europe. Total eclipses of the sun which occur about twice in three years are only to be seen from a very limited portion of the earth's surface. Vague records of them come down to us from the most ancient times, and in the seventeenth and eighteenth centuries a few had been witnessed by trained astronomers on the rare occasions when their tracks had happened to pass over an observatory. But, although total eclipses had frequently been reported to have inspired spectators with admiration, amazement, and even horror, and although certain attendant phenomena had greatly puzzled Halley and other astronomers who had witnessed them, there seems to have been strangely little curiosity as to these matters among the astronomers of the early nineteenth century. Probably, they had taken the general accounts with a " pinch of salt," whereas the astronomical puzzle referred to was felt to be a matter of somewhat minor importance. In any case the stimulus that led to the eager preparations for the 1842 eclipse came from a different direction. In 1836 the moon as seen from Jedburgh had passed centrally across the sun; but, being at the time in the more distant part of its slightly elongated orbit, it had failed to cover the sun completely and left a narrow annulus of the sun's disc unobscured all round it—a so-called " annular eclipse." The event had been observed by the English astronomer Baily, who had noticed a most extraordinary phenomenon. Just before the moon had become centrally placed upon the sun's disc, while one edge of the moon was still in coincidence with that of the sun, there appeared upon this edge for a few moments a string of dazzling beads separated by a number of black ligaments. This appearance, called " Baily's Beads," is now known to be due to a physiological exaggeration by the eye of chinks of sunlight filtering through between the lunar mountains that project upon its edge. But the full explanation was not at that time forthcoming and the discussion to which it gave rise was largely responsible for the enthusiasm with which the 1842 eclipse was awaited.

Expeditions were sent out from many countries to the narrow track of total eclipse. Airy, the Astronomer Royal, went to Turin; Baily himself was stationed at Pavia; Herschel was at Milan; while various foreign astronomers took up positions in Austria and France. It seems that none of these was really prepared for the magnificent spectacle he was to behold; and so it was that the importance of this first of modern eclipses lay not so much in the results it achieved as in the precedent it set for future occasions. It is significant that practically no total eclipse since then has been allowed to occur without every effort being made to observe it, though its duration were never so short and the journey never so distant.

We cannot do better than quote a few extracts from Baily's account of this eclipse as he saw it from the University of Pavia. He had mounted his telescope in an upper room for the purpose of studying the bead formation which he had discovered six years before and which he expected to be repeated in the moments preceding totality. While he was waiting, watching for the phenomenon, he was suddenly " astounded by a tremendous burst of applause from the streets below, and at the same moment was electrified at the sight of one of the most brilliant and splendid phenomena that can well be imagined. For at that instant the dark body of the moon was suddenly surrounded with a corona, or kind of bright glory similar in shape and relative magnitude to that which painters draw round the heads of saints. . . . When the total obscuration took place, which was instantaneous, there was a universal shout from every observer, which 'made the welkin ring,' and for the moment withdrew my attention from the object with which I was immediately occupied. I had indeed anticipated the appearance of a luminous ring round the moon . . . but I did not expect, from any of the accounts of preceding eclipses that I had read, to witness so magnificent an exhibition as that which took place. . . . The breadth of the corona, measured from the circumference of the moon, appeared to be nearly equal to half the moon's diameter. It had the appearance of brilliant rays. The light was most dense close to the border of the moon, and became gradually and uniformly more attenuate as its distance therefrom increased, assuming the form of diverging rays which at the extremity were more divided and of an unequal length. Its colour was quite white . . . and the rays had a vivid and flickering appearance. . . . Splendid and astonishing as this remarkable phenomenon really was, and although it could not fail to call forth the admiration and applause of every beholder, yet I must confess that there was at the same time something in its singular and wonderful appearance that was appalling. . . . But the most remarkable circumstance was the appearance of *three large protuberances* apparently emanating from the circumference of the moon, but evidently forming a portion of the corona. They had the appearance of mountains of prodigous elevation; their colour was red, tinged with lilac or purple; perhaps the colour of peach-blossom would more nearly represent it. They somewhat resembled the snowy tops of the Alpine mountains when coloured by the rising or setting sun . . . their light was perfectly steady, and had none of that flickering or sparkling motion so visible in other parts of the corona. . . . And when the first ray of light was admitted from the sun, they vanished, with the corona, altogether, and daylight was instantaneously restored."

Neither corona nor protuberances were in any sense discovered at this eclipse; what was discovered was that both of them were of the highest scientific interest and worthy of intensive study in the future. The general impression got by the observers concerned was that both corona and protuberances belonged to the sun rather than to the moon; but, being so curiously unprepared for what they were going to see, they were quite unable, in the brief moments available to them, to apply any critical test to settle this point. We now know that the globe of the sun which we see every day, and which has a diameter of about 860,000 miles, is covered by a shallow layer, a few thousand miles deep, of glowing gases which we call the "chromosphere." Now and again gigantic upshoots from this layer may tower like pillars of flame to heights of several hundred thousand miles above the sun's surface—the so-called protuberances or prominences. Above the chromosphere is the tenuous atmosphere of the corona whose delicate curving streamers may on occasion extend to several million miles from the sun's surface. Both chromosphere and corona are entirely drowned by the ordinary daylight, and are only seen at their best during a total eclipse. Since 1868 it has gradually become possible to study the chromosphere and prominences by special methods without an eclipse, but the method cannot be applied to the corona. The study of these appendages has therefore been slow and tedious. About two total eclipses occur every three years; but each eclipse is to be seen from a very small part of the earth's surface, from places that are often distant and sometimes inaccessible. Totality can never last more than 7½ minutes and is usually much less; while a cloud no larger than a man's hand can easily spoil everything. During the last ninety years the corona has been available for observation for an accumulated period of little more than three hours.

A Great Comet. On February 28th, 1843, there suddenly blazed out beside the sun what was probably the most brilliant and interesting comet of the century. So bright was it that it could be seen at midday like a short dagger of light close to the sun. It was not until the middle of March, when it moved into a darker sky, that its presence became known in this country and it was seen as a long silvery beam extending upwards from the fading sunset. Estimates of its length at this time made it at least 200 million miles, equal at least to twice the distance of the earth from the sun. But the most amazing part about this dramatic visitor was the way in which it almost grazed the sun's surface. Travelling at the rate of 360 miles a second it dashed through the very depths of the corona, its outer envelopes missing the sun's surface by no more than 30,000 miles. In the space of two hours it must have been swung from one side of the sun to the other, and in the same interval must have completely reversed not only the direction of its motion but also the direction of its tail. This event was important as exhibiting the excessively rarefied nature of the corona, and as supplying a proof by *reductio ad absurdum* that the tails of comets cannot be permanent appendages. The tail of a comet is always directed approximately away from the sun. If therefore a comet, in the space of two hours, is swung, as a child swings a ball on the end of a string, from one side of the sun to the other, the velocity of the far end of the tail will be not merely inconceivable but impossible. So it was generally felt that the tail must be some kind of an emanation, of an electrical sort, continually being ejected away from the sun out of the comet's head, with a velocity possibly comparable with that of light.

The constitution of a comet's head was scarcely less mysterious. One thing certain was that it was extremely tenuous: the light of the faintest stars shone undimmed through the very centre of it. But there was nothing to indicate at the time whether it was composed of a vaporous material or of multitudes of small and widely scattered particles.

Three comets were already known that returned periodically to the neighbourhood of the sun. The first was the great comet recognized more than a hundred and fifty years before by Halley. He had successfully predicted its return for 1759, and it had come again in 1835; for it revolved round the sun in a period of about seventy-seven years. It was not until 1819 that the second periodic comet was added to the list by the German astronomer Encke. This was a faint comet which had been seen at three previous visits without being recognized as periodic and which took 3½ years to go round the sun—a record for short periods even to the present day. The third was another faint comet with a period of 6½ years discovered by an Austrian officer, von Biela, in 1826.

Certain comets therefore were clearly to be regarded as permanent members of the solar system and not merely as stray visitors from interstellar space. But whether this rule could be applied generally was quite another question. Calculations showed that the 1843 comet we have described moved in a path which it would take over 500 years to go round, while another great comet seen in 1811 would require 3,000 years before it could return again to the sun! But as we shall see later when one comes to periods as long as these and to orbits so vast, it becomes impossible to assign to them any definite figures.

The next great comet was that of 1858 discovered at Florence by Donati. It was remarkable for certain curious physical phenomena which the brighter comets sometimes exhibit; and so once more drew the attention of astronomers to the study of them, Thus instead of a single tail it possessed, in one stage of its development, two: a strongly curved tail opening like a fan outwards from the head; and a narrow beam-like streamer which, instead of sharing the curvature of the main tail, stretched in a straight line directly away from the sun. Again the structure of the head was complex and variable; for there appeared as brighter condensations in the diffuse material, a succession of arches, or "envelopes," concentric with the central nucleus and on its sunward side. It appeared as though matter was ejected towards the sun and was then turned back, by some repulsive force of solar origin, to stream away into the tail—just as water rising from a fountain curves down again in numerous arching sprays. Such phenomena had been observed in earlier comets, and already an explanation of them had been put forward by Heinrich Olbers, the physician-astronomer of Bremen. He had suggested that the small particles, of which he supposed the comet was to some extent made up, were subject to repulsive forces exerted both by the comet's head and by the sun. Some of these would be repelled at once straight into the tail in a direction away from the sun; others would at first be repelled by the head towards the sun, and only subsequently be turned back by the solar repulsion so as to curve round and join the tail. He had also pointed out that the particles might be of different kinds with varying susceptibilities to the repulsive forces. The most susceptible would be repelled with the largest velocity and form the straight tails, while those repelled more slowly would form tails of greater curvature.

The two comets which were responsible for the most significant advance in our knowledge of the nature of these bodies, were two comparatively unimpressive-looking objects that approached the sun in 1862 and 1866 respectively. For they led to the discovery of the intimate relation between comets and shooting stars. In the beginning part of the century the views current about shooting stars were extremely vague. Their heights above the ground had been roughly determined, and their velocities had been shown to be comparable with those of planetary bodies. It was therefore presumed that they were particles which entered our atmosphere from outer space; but what they were doing in outer space was very uncertain. Many including the great Laplace thought they might be small volcanic bodies ejected from the moon. But in 1833 astronomers began to take shooting stars much more seriously, for on November 12 in that year there was observed in America a shower of shooting stars the intensity of which was quite unexampled. The number seen during the night, it was estimated at Boston, must have approached a quarter of a million. In particular it was noticed that all these " stars " appeared to radiate outwards from one quite small region of the sky; and this it was shown must be the effect of perspective on the motions of bodies which were in reality travelling in exactly parallel paths. It followed that the shooting stars had been produced by a large number of particles which prior to their submersion in our atmosphere had been travelling together through space in a swarm.

The other significant point which was brought to light as a result of the interest aroused by the great shower, was that it had occurred though in a much attenuated form on the very same date in previous years. Moreover, in 1836, Quetelet of Brussels was able to announce that there was another date in the year, namely August 10, round which shooting stars were unusually abundant. In fact it had long been a popular tradition that on this day of St. Lawrence the tears of the martyr were to be seen falling in the sky. Thus two showers of shooting stars could definitely be regarded as periodic, repeating themselves annually about the same date. From these facts certain definite ideas gradually took form: (1) that the particles responsible for a shower of shooting stars revolved as a swarm in an orbit round the sun; (2) that the orbit happened to intersect the earth's orbit at a certain point—in the first case the point at which the earth was in November, and in the second the point where it was in August; and (3) that the swarm instead of being limited in size and spherical in shape extended more or less continuously in the form of a ring all round the orbit.

In 1864 another discovery was made by H. A. Newton of Yale. In searching through ancient records he found references to unusually fine displays of shooting stars occurring about November as far back as A.D. 902. The references came at intervals of thirty-three or thirty-four years or simple multiples of that period. It seemed therefore that the swarm of the November meteors though to some extent distributed in a continuous ring round the orbit, still retained a proportion of its component particles in a fairly compact mass. The period in which both the compact and diffuse parts of the swarm revolved round the sun had to be such that the compact part would hit off the earth every thirty-three or thirty-four years. Newton concluded that it might be any one of five widely different periods including the period of 33¼ years, but it was left to Adams, the discoverer of Neptune, to decide between them and show that the 33¼-year period was the correct one.

In 1866 two things happened of the greatest importance. The lesser event was a repetition of the great shooting star shower of November 1833. It was important as an event in itself; but it was more important as providing a powerful stimulus for meteoric study—for this time Europe was favoured by the spectacle and astronomers were ready prepared for it by Newton's prediction. The other event was the announcement by Schiaparelli in Milan, after an exhaustive inquiry into the matter, that the August meteors moved round the sun in an orbit identical with that of the comet of 1862, the first of the two comets last mentioned. There could be little doubt that the comet was actually a part of the meteor swarm, presumably the most concentrated part of it. The next year the certainty of this conclusion was greatly enhanced through the independent discoveries by Leverrier and Schiaparelli that the November meteors travelled in the same orbit as the comet of 1866, the second of the two comets mentioned. It was particularly significant in this case that the great shower should have occurred at our *first* crossing of the comet's orbit after the comet had itself passed by: for the comet was visible in the early part of the year, whereas we encountered the meteors in the November of that year. Presumably if we had arrived at the crossing some months earlier and actually hit the comet off, the display would have been even finer than it actually was. How much finer it would have been and whether it would have been dangerous it is impossible to say.

Telescopes. We must now leave the heavens for the workshop and see what was being done during this period in the construction of telescopes. Although the reflecting telescope had made enormous strides in the hands of Herschel and had culminated in a monster over four feet in diameter, the achromatic refractor of Hall and Dollond was still limited, owing to the impossibility of obtaining optical glass, to diameters of three or four inches.

About the time when Herschel was making his mirrors in England, Pierre Louis Guinand, by trade a maker of small bells living in the canton of Neuchatel, started to make lenses for his own interest. From England, which was at that time the only source of supply, he obtained some flint glass; but finding it to be of inferior quality he set about trying to make some for himself. After several years of failure, during which he practically reduced himself and his family to poverty, he began to be successful. And in 1799 he was able to take with him to Paris some flawless discs of flint glass up to six inches in diameter. The like of them had never been seen before, and during the next few years the fame of this humble Swiss glass-maker spread widely. In 1805 he was persuaded to go to Munich to be attached there to the recently founded Optical Institute, where he was destined to instruct and work with perhaps the greatest optician of all time.

In 1801 there had collapsed in Munich an ancient house which buried in its ruins a fourteen-year-old orphan, Joseph Fraunhofer. Though badly injured the boy was rescued. With a small sum of money presented to him by the Elector Maximilian Joseph, who had happened to be a witness of the accident, he bought for himself some books on mathematics and optics and a machine for polishing glass. And finally after a few years of hardship and poverty, during which he educated himself in theoretical and practical optics, he joined in 1806 the Munich Optical Institute—a year after the arrival of Guinand.

To this fortunate association of Guinand and Fraunhofer we owe the optical glass industry both of Germany and of the world, as well as the achromatic telescope in its perfected form. Guinand continued to improve his art and divulged the secrets of it to Fraunhofer; while the latter not only became expert in the art of glass making but pursued with increasing success his experiments in the figuring of lenses and in obtaining the most suitable combinations of their curvatures and their densities for the production of optical images.

In 1824 Fraunhofer produced what was then by far the largest and most perfect achromatic telescope in the world, one 9½ inches in diameter which was procured by the Russian Government for the Dorpat Observatory. For this achievement Fraunhofer received a title of nobility and was appointed to the management of the Optical Institute. Soon afterwards he designed and constructed that magnificent instrument for the observatory at Königsberg with which Bessel measured the distance of the star 61 Cygni. Fraunhofer accomplished an incredible amount of work in a very short time; for in addition to perfecting the achromatic telescope he laid the foundations, as we shall see later, of astronomical spectroscopy. Yet, when in 1826 he died of tuberculosis, he was only thirty-nine years old.

In 1814 Guinand had returned to Switzerland to continue his work. There he produced the 14-inch and 12-inch discs for two telescopes constructed in Paris. He died in 1824, but the art which he had created lived on and developed after him. In Munich it flourished in the hands of Fraunhofer and his successors; to Paris it was brought by his own son and has there been handed down through successive firms up to the present day; and finally during the revolution of 1848 it came from Paris to England, to the famous firm of Chance of Birmingham.

Thus although English opticians had led the world in the development of the achromatic telescope during the eighteenth century they contributed practically nothing towards its advancement during the first half of the nineteenth century. The scene of activity had shifted to Switzerland, Germany and France. We have already seen that one reason for this was that the secret of glass making had not yet reached us; but another reason was that the heavy duty imposed upon optical glass prevented our opticians from purchasing it from abroad. Thus in 1840 when the Munich firm finished for the Pulkowa Observatory a lens fifteen inches in diameter, the largest lens produced in England had a diameter of only six inches.

But if our country was backward in the art of glass making, it made up for the defect by producing another great amateur astronomer to continue the pioneering work of Herschel in the construction of great reflecting telescopes. In 1845 the third Earl of Rosse completed a gigantic mirror six feet in diameter. Since 1827, in the park of Birr Castle, Parsonstown, he had carried out, in home-made workshops and with home-trained mechanics, laborious experiments on suitable metal alloys and on problems of grinding and polishing. Finally in February 1845 the " leviathan of Parsonstown "—its 54-foot tube swung rigidly between piers of masonry fifty feet high by seventy long—reared its monstrous mouth heavenwards. The main contribution made by this telescope to astronomy was its revelation of the structure of the nebulae and star-clusters. At first it seemed that this instrument of unrivalled power would resolve all the so-called nebulae—previously thought to be vaporous in nature—into clouds of star-dust, and demonstrate that the filmy material, which some had thought to be the primordial stuff out of which stars were born, was merely an illusion due to distance. But, although this view was adopted by many at the time, others, including Lord Rosse and his assistants, maintained a more conservative attitude. The most important series of discoveries, however, began in the third month of the telescope's existence with the detection near the tail of the Great Bear of a veritable celestial whirlpool. There, delineated in hazy outlines, was a delicate spiral structure of almost geometrical precision. This was the first nebula shown to be spiral, but in the years following many more of Herschel's filmy objects were shown to have a similar make-up. It is very odd that this particular conformation, so lavishly employed by nature in the structure of sea-shells, should also be found to be the fundamental architectural plan in the most stupendous objects of creation. There were, however, more than eighty years to go before the spiral nebulae were to form the subject of one of the most astonishing theories ever conceived.

CHAPTER II

DISCOVERY OF NEPTUNE, VARIABLE STARS AND THE BEGINNINGS OF STELLAR SPECTROSCOPY

THE year 1846 saw what was probably the most daring mathematical enterprise of the century brought to fruition. Two mathematicians independently predicted the existence and precise position in the sky of a new member of the solar system; and, when the telescope was directed to the spot, the object was actually there.

The first addition to the solar system in historical times had been the telescopic discovery by Herschel of the planet Uranus in 1781. Rather before this, a curious regularity had been noticed in the disposition of the planetary orbits, a striking exception to the " rule " being an obvious gap between the orbits of Mars and Jupiter. This still unexplained regularity of arrangement, generally known as Bode's Law, fired the enthusiasm of a small group of astronomers who determined to " police " the skies and track down the missing and hypothetical planet. By 1807 their activities had triumphed to such an extent that the vacancy had been filled not with one but with four (though rather insignificant) objects. At the time it was suggested that they represented a planet that in the past had been exploded into fragments. Since the innovation of photography the further discovery of similar objects has grown thick and fast, their numbers having now reached four figures; a few of the largest have diameters of two or three hundred miles, but the majority are very much smaller. They are generally known as the Minor Planets.

Discovery of Neptune. But in an entirely different category of discoveries is that of the planet Neptune with which we must now concern ourselves. Neptune is a major planet of the same order of size as Uranus and the five ancient planets; moreover the manner of its discovery was unique.

The planet Uranus had not been behaving as it should. After its discovery in 1781 old records of its position (measured, of course, on the presumption that it was an ordinary star) were traced back to 1690; but no orbit could be calculated to fit them exactly. Even when these earlier positions were rejected as presumably erroneous, things were scarcely any better; Uranus continued to move in a manner entirely unbecoming to a planet governed by Newtonian laws. Ultimately matters reached such a pitch that the suggestion was made that there was another planet beyond Uranus whose gravitational attraction was causing it to stray from its rightful path.

Early in 1843 the young mathematician Adams, so soon as he had taken his degree as senior wrangler, started to work out the problem whether these irregularities could be produced by a trans-Uranian planet, and, if so, what position in the sky the body would have to occupy in order to produce the observed effect. This amazing task, like which nothing had ever been attempted before, was completed in the autumn of 1845; and the results were communicated to Airy, the Astronomer Royal. Airy seems to have failed to appreciate the full importance of the matter. After all he was a very busy man, and Adams with his retiring disposition had not exactly " pushed " his work nor even offered much evidence of its authoritative character. So Airy laid it aside practically unattended to. It was not until the following June that Airy's attention was again drawn to the matter by a paper on the same subject from the pen of a young French mathematician, Leverrier. So similar were the conclusions of the two papers that Airy, realizing that there must after all be something in the prediction, wrote to Challis at the Cambridge Observatory asking him to make a search in the region of the sky specified. Challis unfortunately had no map of the fainter stars in this region; he had to prepare his own map of the stars in the neighbourhood, and wait to see which one of them would give away by its motion the secret of its planetary nature. By October 1 he had measured the positions of over 3,000 stars and was just thinking of plotting them on a chart, when news arrived from Berlin that Galle had found the planet on September 23. Only that same day Galle had been asked by Leverrier to look for the planet, and, being in possession of an excellent map, he had found it after a very brief search. So the honour of the mathematical achievement was shared between the Frenchman and the Englishman, while the credit for the observational discovery went to the German who was ready for, and prepared to take quick advantage of, an excellent opportunity.

Sun Spots. While the investigations we have been describing were being prosecuted with the most delicate of measuring instruments, with giant telescopes, or with the latest refinements of mathematics, a German apothecary, Heinrich Schwabe by name, was indulging the simple hobby of counting daily with a small telescope the spots upon the sun. In 1843, after seventeen years of this routine, he perceived that something important was materializing from it: a definite cycle in the number of spots visible. But the world was not as yet impressed; and only in 1851, when his labours had been carried over a quarter of a century, did his results receive recognition. Sun-spot activity undoubtedly waxed and waned in a period of about ten years. Almost at the same time two other investigations of quite a different sort bore fruit. Observations in Germany of the daily fluctuation of the magnetic needle showed that it increased and diminished in amount in a 10-year period. The Toronto records of magnetic storms—violent, spasmodic agitations of the magnetic needle distinct from its regular daily fluctuation—showed a periodicity in their violence and frequency, again, of ten years. These three results were reached independently; and it was Sir Edward Sabine, who had investigated the Toronto records, who first pointed out the identity of their periods and, most remarkable of all, the fact that the three phenomena were precisely in step, rising and falling together in unison. Soon after, yet a fourth phenomenon was included in the association. The Aurora Borealis, which, often in the Polar regions and rarely in this country, adorns the northern horizon at midnight with flickering streamers and coloured curtains of light, had long been known to be accompanied by a sympathetic agitation of the magnetic needle; and an examination of the records made it clear that the frequency of these displays was also to be correlated with sun-spot activity.

When in 1852 the discovery of the connection between the periodicities of sun-spots and magnetic storms was made known, one Richard Carrington was in process of erecting a private observatory at Redhill in Surrey. The interest of Carrington was aroused; and he began, as soon as his observatory was completed, to make daily measurements of the positions of the spots upon the sun's surface. Owing to the death of his father, which entailed his taking over the management of the family brewery, and subsequently to his own ill-health and early death, the observations which were begun in 1853 were brought to an end in 1861. These eight years were however sufficient for Carrington to make two discoveries of the greatest importance and to determine with an accuracy hitherto unattained the period of the sun's rotation on its axis. He found that the sun did not spin on its axis as a solid globe. Instead the parts of its surface nearest to its equator travelled more rapidly and rotated in a shorter period than did those parts of its surface situated in higher latitudes north and south of its equator. He adopted as the average period of the sun's rotation one of twenty-five days, nine hours, and found that this represented the rotation period of sun-spots in north and south latitudes fourteen degrees. For spots actually on the equator the period was slightly less than twenty-five days, while for spots in north and south latitudes fifty degrees it was as much as 27½ days. These results were announced in 1859, and two years later practically similar conclusions were arrived at independently by Gustav Spörer of Berlin.

It had been known for some time that sun-spots were practically confined to two narrow zones on either side of the sun's equator lying between north and south latitudes thirty-five degrees and six degrees. Spots occur occasionally nearer to the equator than six degrees and sometimes even on the equator; but spots are very rare in latitudes higher than thirty-five degrees and practically never occur in latitudes above fifty degrees. Carrington found that as the 10-year cycle[1] of solar activity approached its minimum, the two spot zones contracted down towards the equator while the spots themselves gradually petered out. Then suddenly a few spots would begin to appear in high latitudes; for the next few years their numbers would gradually increase, while at the same time the two zones in which they appeared would converge slowly upon the equator.

When one realizes how transient sun-spots usually are, one cannot help admiring the way in which Carrington achieved so much in so short a time. For many sun-spots close over again within a few days of their first appearance; and only occasionally are they large enough to survive through several rotation periods and make repeated appearances on the sun's visible hemisphere.

Stellar Brightness and Variability. That one or two of the stars in the sky fluctuated in brightness had long been known. The variability of Algol, or the Demon, was probably known to the Arabs; and the variability of Mira, a star in the constellation Cetus, was discovered by the Dutch astronomer, Fabricius, in 1596. Nevertheless, at the close of the eighteenth century there were only eleven stars of which the variability had been definitely confirmed. It was in the last few decades of that century that the systematic study of variable stars really began. The pioneers in this field were two young men, Goodricke and Pigott, who were close friends and observed together in York. Goodricke made the first complete study of the variation of Algol. From the manner in which it varied and its perfectly regular periodicity he deduced the fact, long afterwards confirmed, that it periodically suffered eclipse by a dark star revolving round it. In addition Goodricke discovered and investigated two other variable stars; yet he died when he was only twenty-two and he was born deaf and dumb. Pigott, who survived to the age of thirty-nine, discovered and studied three new variables and published a catalogue of fifty stars known or suspected to be variable. The work of these two not only gave an impetus to variable star astronomy but turned the attention of astronomers to the important question of the determination of stellar brightness.

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[1] Modern observations make this period about eleven and a half years.

About 130 B.C. Hipparchus had arranged the naked eye stars in six classes of brightness, or " magnitudes." The brightest stars were classed as being of the 1st magnitude; those which were just on the limits of naked eye visibility were classed as being of the 6th magnitude. Two and a half centuries later Ptolemy found he was able to divide up each of these classes into three subdivisions and thus give greater precision to the figure specifying the brightness, or magnitude, of a star. Sixteen centuries then elapsed before any serious improvement was made on Ptolemy's determinations. Towards the end of the eighteenth century the elder Herschel arranged the stars in the different constellations in the order of their brightness. His lists included some three thousand stars and formed the earliest really accurate record of the comparative brightness of the stars.

At the beginning of the nineteenth century the new study of stellar variability brought the whole problem of the measurement of stellar brightness very much more to the front. Many of the most eminent observers became interested in it, for it now seemed that the relative brightness of the stars deserved to be measured and recorded as much as their relative positions in the sky. The magnitude determinations handed down from Ptolemy were everywhere in general use, and in order to avoid the unutterable chaos which would otherwise have resulted, each observer adopted in general outline the classical scheme and built up upon it his own more detailed magnitude scale. At that time the only practicable way of measuring the relative brightness of the stars was by eye estimates. An observer constructed in different parts of the sky " sequences " of stars from the brightest to the faintest, and selected them as far as possible so that a given star in the sequence differed from the one immediately brighter or fainter than it by a barely perceptible amount. The various sequences were then linked up by cross sequences. Finally a magnitude scale was fitted to the sequence by adopting as closely as possible Ptolemy's magnitudes for such stars in the sequence as they were available for. Naturally however the fit could not be perfect and many compromises were necessary; and as a result the magnitude scales of the various observers differed considerably from one another. Oddly enough the differences were not nearly so large as one might have supposed; and tables of corrections have been formulated by which the magnitudes of one observer can be converted into those of another.

It was clearly desirable that this confusion should be done away with and that the magnitudes of the stars should be definitely standardized. The earliest attempt at this was made by the younger Herschel while he was at the Cape. He compared the brightness of a large number of stars with an artificial point of light the brightness of which could be varied by a known amount. He also compared a bright star seen in a small telescope with a faint star seen in a large telescope; and since the brightness of a star in a telescope is proportional to the *area* of the telescope lens, he was able to arrive at the relative brightness of the two stars. By means of these two methods he found that an average star of Ptolemy's 1st magnitude was about one hundred times brighter than an average star of Ptolemy's 6th magnitude, and that the abnormally bright star Sirius was about three times as bright as an average 1st magnitude star. By 1850 several observers by similar methods had arrived at closely the same result: Steinheil (whose work actually antedated that of Herschel) and Argelander in Germany, F. G. W. Struve and Otto Struve in Russia, and Johnson and Pogson at Oxford. Finally about 1854 Pogson, who was then assistant at the Radcliffe Observatory, Oxford, made the important suggestion that since a difference of five magnitudes was so closely represented by a difference in brightness of 100-fold on the scales of the various observers, these convenient figures should be adopted as the basis of the definition of magnitude. If a step of five magnitudes represents 100-fold change in brightness, then a step of one magnitude represents a change in brightness of 2·512 times. This figure is known as "Pogson's ratio." It means that a 1st magnitude star is 2·512 times brighter than a 2nd magnitude star, a 2nd magnitude star 2·512 times brighter than a 3rd magnitude star, and so on. It means that a 1st magnitude star is exactly 100 times brighter than a star of the 6th magnitude which is just visible to the naked eye, exactly 10,000 times brighter than a telescopic star of 11th magnitude, and exactly a million times brighter than a telescopic star of the 16th magnitude. But though this convenient definition of Pogson was soon adopted in theory, it was a very long time before the actual technique of measuring the brightness of the stars had achieved sufficient precision for it to be adopted with any accuracy in practice.

The outstanding figure in the early history of the determination of stellar magnitudes and of the study of variable stars was F. W. A. Argelander, who was born the son of a wealthy merchant at Memel in 1799. At the age of twenty-two he became Bessel's assistant at Königsberg and for two years worked with him on his experiments for charting all the stars down to the 9th magnitude—a limit about fifteen times fainter than that visible to the naked eye. In 1823 he became director of the observatory at Abo, and in 1836 was appointed to direct the new observatory which was to be built at Bonn. It was during his early years at Bonn, while awaiting the completion of the observatory, that Argelander first became interested in variable stars, and made the observations for his *Uranometria Nova* which consisted of charts giving all the naked eye stars visible in Europe and a catalogue listing their magnitudes and approximate positions. But this was only the prelude to his great work, which was to carry through the scheme contemplated by Bessel of charting and cataloguing all stars down to the 9th magnitude. And to this he now added the new plan of making it a catalogue not only of positions but also of magnitudes. The observations were begun in 1852, and with his assistants, Schönfeld and Krueger, he laboured so persistently that the whole thing was published by 1863. This famous catalogue with its charts is known as the *Bonner Durchmusterung*; it gives the positions and magnitudes of no less than 324,198 stars based on the results of over a million and a half observations. It was by far the most comprehensive survey of star positions and magnitudes made up to that time; and even at the present day it is probably more frequently used than any other chart and catalogue. No charts made visually have ever equalled it; and since photographic charts show red stars fainter than they *look*, a photographic chart is not very convenient for identifying stars *seen* with a telescope. Again although there are modern catalogues giving much more accurate determinations of magnitudes these have not yet been extended to such faint stars as are included in the *Bonner Durchmusterung*, except in certain special regions of the sky.

But though this building up of a solid and permanent structure of stellar magnitudes was perhaps Argelander's most significant contribution to astronomy he did much more. He was the first to get the study of variable stars going on a really sound basis. He improved the method devised by William Herschel, by which the brightness of a variable star can be determined with a considerable degree of accuracy from simple eye estimations, in which it is compared with stars of *known* brightness close to it slightly brighter and slightly fainter than itself. Finally he was the first to realize that if the study of variable stars was to make any headway it would be necessary to gain the co-operation of many more observers than could be spared from the ranks of the professional astronomers. So in 1844 in Schumacher's *Jahrbuch*, he published " An Appeal to the Friends of Astronomy ":

> " . . . Therefore do I lay these sorely neglected variables most pressingly on the heart of all lovers of the starry heavens . . . the observations may seem long and difficult on paper, but are in execution very simple—I am thoroughly convinced that whoever carries on these observations for a few weeks will find so much interest therein that he will never cease. I have one request, and it is this ; that the observations shall be made known each year. Observations buried in the desk are no observations. . . ."

As the years passed and the rate of discovery of variable stars increased the need of assistance from the amateur to keep these objects under regular observation became vastly more urgent. We shall see later that this appeal first issued by Argelander has been answered with ever increasing enthusiasm, so that at the present time there is a large body of amateurs scattered all over the world making regular measurements of the brightness of these stars.

Celestial Chemistry. In 1861 the German chemist, Kirchoff, presented to the Berlin Academy a remarkable chart nearly eight feet long, which showed beyond all manner of doubt that there were present in the sun's atmosphere many ordinary terrestrial substances such as iron, hydrogen, and sodium. This event may be regarded as the official opening of the new science of celestial chemistry. It is out of place here to tell the fascinating story of the birth of spectrum analysis, of its numerous abortive attempts to enter the world before it was finally delivered by the painstaking labours of Kirchoff and Bunsen.

White light, as Newton long ago showed, is a combination of all the colours of the rainbow. It is best broken up into its component hues by passing it through a piece of glass cut in the form of a triangular prism. Nature frequently does the same thing for us by passing the sunlight through raindrops when the sun returns after the rain and the rainbow is seen upon the departing shower.

If a large piece of cardboard with a narrow slit in it is placed in *front of an electric lamp*, and if the illuminated slit is then examined with a glass prism it will be seen to be broadened out into a wide band of colour from violet at one end, through blue, green, yellow and orange, to red at the other. In other words the electric lamp emits all the colours of the rainbow, and the band that is seen is made up of an infinite number of different coloured slits side by side, each one overlapping the next. If now the slit is illuminated by the glowing vapour of some volatile substance—a mercury or " ultra violet " lamp serves the purpose—a very different effect is produced. Instead of an infinite number of overlapping slits forming a continuous band of colour, there are visible about half a dozen separate slits each of a different colour side by side—the violet slits to one side, then the blue, green and yellow slits, but for mercury no orange or red slits. Every chemical substance suitably made to glow emits colours that are characteristic of it. The sodium flame, got simply by putting a spot of table salt in a spirit lamp, gives but two yellow lines very close together. Iron, on the other hand, the flame of which can be obtained by striking an electric arc across two iron rods, shows thousands of coloured lines. So a " spectroscope," an instrument consisting of a prism and slit *plus* lenses to increase the brightness and length of the spectrum, can be used to examine what elements any particular substance is composed of.

But a glowing vapour not only *emits* its characteristic colours; it can also absorb those same colours. Let the slit and prism be arranged as before and let the slit be illuminated by the flame of common salt. There will then be visible the two yellow lines of sodium. But if now the electric light is made to shine *through the sodium flame* on to the slit, a complete metamorphosis occurs. The continuous spectral band of the electric light is now visible, but, superimposed upon it, and exactly where the yellow lines previously were, are now two black lines (really gaps) in the yellow part of the band. Thus the glowing sodium vapour has absorbed its own particular colours from the spectrum of the electric lamp.

Now several years before, Fraunhofer, the great lens-maker of Munich, had allowed a sunbeam to enter a room through a slit and had examined that sunlit slit through a telescope and prism. The brilliant coloured band which he saw was traversed by numbers of fine dark lines, the positions of which he measured. These lines thereafter were called " Fraunhofer's Lines," but it was Kirchoff who now explained what they meant. By means of experiments such as the one described, he proved that the light from the incandescent surface of the sun was robbed, in its passage through the solar atmosphere, of those particular colours characteristic of the chemical elements in that atmosphere. Thus the lines in the solar spectrum told us what substances existed in the sun's atmosphere. And it was this that upon the eight-foot map he had so convincingly proved. Besides the central strip delineating the positions of myriads of solar lines were charts of the lines of corresponding elements; so numerous and exact were the coincidences thus exhibited that the chances of their being merely fortuitous were estimated by Kirchoff as being but one in a trillion!

It was an obvious step from the sun to the stars, and the spectroscope was not slow to take it. The first in this field were Father Secchi in Rome, one of the many eminent Jesuit astronomers of the century, and Sir William Huggins at Tulse Hill, one of the greatest of British amateur astronomers. The work of Secchi was more comprehensive than detailed; he made the first spectroscopic survey of the sky comprising a review of the spectra of several thousand stars. The result was to demonstrate a grouping into four main types, characterized both by the colour of the star to the eye and by its spectral appearances. The white stars, including many of the brightest in the sky, showed a number of broad dark lines indicating the predominance of hydrogen in their atmospheres. The yellow stars gave spectra generally similar to that of the sun, the hydrogen lines less broad and prominent, and the multitudinous lines of the metals forming the striking feature. Finally, his third and fourth types included the red stars, whose complex fluted bands demonstrated the presence of molecules rather than atoms: for, apparently, in the cooler atmospheres of such stars, not white but only red-hot, the temperatures were low enough to permit of some of the atoms entering into actual chemical combinations.

Huggins, on the other hand, with the more powerful apparatus of his private observatory, was concerned with the more detailed examination of a smaller number of stars; and, by making careful comparisons between the lines they exhibited and those of known terrestrial elements, he gave us the first detailed answer to the question, "what are the stars made of?" On August 29, 1864, Huggins made a discovery of paramount importance. Turning his spectroscope upon a bright nebula in the constellation of the Dragon, he perceived that, instead of being drawn out into a band of light, it emitted one solitary green ray. A spectrum consisting of one or more *discrete* coloured rays is, as we saw above, characteristic of light emitted by a glowing gas. Thus, by an observation which was direct and final, Huggins abruptly ended the long dispute as to whether any of the so-called nebulae were really composed of clouds of gas, or whether all of them would, with sufficient telescopic power, become resolved into clusters of distant stars. During the next four years out of seventy nebulae examined Huggins found about twenty that were truly gaseous; the remainder, which gave a continuous spectrum, were possibly remote systems of crowded stars.

Total Eclipses of the Sun. The eleven-year period 1860–71 witnessed an array of total solar eclipses of the greatest historic interest. In 1860 an eclipse visible in Spain was observed with the new method of photography by Father Secchi and the English astronomer Warren de la Rue. The photographs obtained showed definitely that as the dark body of the moon advanced the prominences were successively concealed upon one side and uncovered upon the other. There could no longer be any doubt that the prominences belonged to the sun and not to the moon.

In August 1868 the sun was totally eclipsed for 5½ minutes from a narrow track of country crossing India and the Malay peninsula. Four expeditions, two French and two British, were stationed at different places, all with the purpose of observing the event spectroscopically. A large prominence happened to be present on the sun's edge; and in the four sets of spectroscopes immediately directed to it a small number of dazzling coloured lines blazed forth. As had been expected, the prominences were thus shown to consist of glowing vapours, among which the coloured rays of hydrogen were at once recognized, while an additional yellow ray was at first thought to represent sodium. But the chief event was the impression which the dazzling brilliance of the prominences made upon the French observer Janssen: surely he thought, the lines ought to be visible apart altogether from an eclipse. The next morning he carried his idea into effect; and when the slit of his spectroscope was placed precisely upon the sun's edge the bright lines of the previous day's totality flashed out in their brilliant colours upon the background of the solar spectrum. From that day on it was possible to study the chromosphere and prominences without an eclipse. As a matter of fact, this same technique had been foreseen, though not realized, by Huggins in the previous February; while, quite independently, Sir Norman Lockyer had actually ordered a more powerful spectroscope for this same purpose. So soon as the instrument arrived, in October of that year, Lockyer also discovered the lines; and on the same day as he made the announcement Janssen's news arrived from the East.

The principle of the daylight view of prominences is quite simple. Whereas the light from the sun's disc, and from the sky generally, is spread out more or less evenly through all the colours of the rainbow, that from the prominences is concentrated in a few narrow strips of colours; so, by isolating one of these strips, one gets a considerable percentage of the light of the prominences and at the same time reduces to a minimum the diffused skylight. It is best to use a curved slit, rounded to fit precisely the edge of the image of the sun formed in the telescope. When the slit is placed just within the edge of the sun one sees the Fraunhofer lines dark as usual, but *curved* because the slit is curved. As the slit is moved so as to lie along, and *just outside*, the edge of the sun, most of the dark lines fade out while one or two, including the hydrogen lines, become suddenly bright. If the opening of the slit is now gradually widened the bright curved lines grow broader and become somewhat jagged on their convex borders. Actually, the curved bright lines are images of the bright chromospheric layer lying round the sun's edge, and if a prominence happens to be in process of erupting from it, one will see its tree-like form standing out on the sides of the curved lines.

From now on the spectroscopic study of the chromosphere and prominences became part of the daily routine of a number of observatories; and our knowledge of them, previously snatched from infrequent and hasty glimpses caught during eclipses, grew rapidly as a result. The chromosphere turned out to be a constant feature surrounding as a shallow layer, several thousand miles deep, the entire circumference of the sun. From this occasionally the prominences, huge flaming columns, reared themselves up. Sometimes, in the space of a few hours, they would be ejected to heights exceeding a hundred thousand miles. Besides them the earth could be likened to a pea licked by the flames of a large fireplace. They would resemble, on different occasions, Cleopatra's Needle, or an oak tree with wide spreading branches, or the tall arches of some Gothic cathedral. That they often made their appearance immediately over a sun-spot which was near the sun's edge, suggested that their eruption was due to violent upheavals beneath the sun's surface.

Another result of the leisure that had been achieved for the study of these objects was that the yellow ray in the chromospheric spectrum, too hastily identified with sodium at the recent eclipse, was found to be distinct from that of any known terrestrial substance. Lockyer called this new element *Helium*, to commemorate the fact that its existence was first recognized upon the sun, Nearly thirty years were to elapse before this same gas, so important nowadays for the inflation of airships, because of its uninflammability and lightness, was detected on the earth by Ramsay.

The North American eclipse of the following year, August 1869, was famous for the discovery of a strange green ray in the spectrum of the corona. This was the brightest and first discovered of a number of coronal radiations possessing no known terrestrial counterpart, and believed till quite recently to indicate the existence of yet another new element in the sun, to which the name *Coronium* was given.

In December 1870 the shores of the Mediterranean were favoured for two minutes by a total eclipse. But despite the shortness of the duration the excitement of the eclipse fever coaxed thither expeditions from all over the world. Unfortunately a cloudy sky greeted most of the observers. Several expeditions which came all the way from America saw nothing; Lockyer, after suffering shipwreck en route, caught a glimpse of only a couple of seconds; while Janssen, who went so far as to escape in a balloon from besieged Paris, saw nothing at all. However, the achievement of Professor Young from Princeton more than made up for the disappointment of the rest. With the slit of his spectroscope trained upon that edge of the sun where the advancing body of the moon would complete its total obscuration, he watched the Fraunhofer spectrum fade from view until suddenly, at the moment of totality, "as a bursting rocket shoots out its stars," the whole field of view became momentarily filled with countless brilliant lines. This was the first observation of the so-called "flash spectrum" of the *reversing layer*. This spectrum, like that of the chromosphere, consisted of bright lines, but their number, instead of being limited to about a dozen, was literally legion. This spectrum was, as its name implied, a replica in reversal of the ordinary Fraunhofer spectrum. The reversing layer is the true solar atmosphere which is responsible through its absorption for the dark lines in the sun's spectrum. It is only about a couple of hundred miles deep and forms the lowest layer of the chromosphere. The main part of the chromosphere lying above it contains only those few gases of great buoyancy which rise to the top. It is because the large majority of the solar gases are confined so strictly to the bottom of the chromosphere's fiery ocean, that the reversing layer is at once so important and yet so difficult to observe. Its extreme narrowness and immediate proximity to the sun's surface make its observation out of eclipse vastly more difficult than that of the chromosphere; on the other hand, during an eclipse it is covered up, almost as soon as isolated, by the advancing body of the moon. Thus it was not until photography had been brought to greater perfection and was able to make an instantaneous and permanent record of it, that the spectrum of the reversing layer could be satisfactorily studied.

The last of this series of five eclipses swept across India and Australia in December 1871. The chief prize was carried off by Janssen ,who detected in the faint coronal light the ordinary Fraunhofer spectrum. Clearly, therefore, the corona, in addition to emitting its own peculiar light, consisted of particles which were capable of reflecting back to us the light of the sun.

CHAPTER III

THE SUN'S DISTANCE AND EARLY WORK ON THE PLANETS

Sun's Distance. In 1874 an event was to occur which it was hoped would settle once and for all a considerable uncertainty as to the distance of the sun. The planet Venus was due to make an appearance in silhouette upon the disc of the sun. These " transits " of Venus are rare events. Venus overtakes us and passes between us and the sun once every nineteen months; but generally, owing to the slight tilt of her orbit, she fails actually to hit off the sun, passing a little above or below him. But now and again a " bull's eye " is scored and Venus is to be seen as a small black spot crossing the sun's disc. These events usually occur in pairs at intervals ranging round 120 years: it had happened in 1761 and 1769, and was due to happen in 1874 and again in 1882.

Now, if one can only find the distance from the earth of any one of the planets, the distances of all the others, from each other and from the sun, will immediately follow from Kepler's laws. Thus Cassini in 1672, by measuring the distance of Mars when nearest to us, had found the sun to be about eighty-seven million miles away. The whole point is to select a planet that comes very close to us, because the nearer a body is, the more accurately its distance, and, therefore, the distance also of the sun, can be determined. One might have thought that Venus, coming nearer to us, would have been a more suitable object for the purpose. But usually, when Venus is nearest us, she lies between us and the sun and is drowned by his radiance. On the rare occasions of a transit, however, things should be different; and it was thought that the black disc of Venus projected against the sun would offer an ideal subject for measurement when viewed from distant points on the earth's surface. Hence the excitement that awaited the event of 1874.

Now there are two other methods, independent of the direct measurement of a planet's distance, by which the sun's distance can be found. The first depends, through the law of gravitation, upon certain irregularities in the moon's motion; while the second involves measuring the velocity of light.

The simple motion of the moon round the earth is disturbed in a highly complicated manner by the gravitational attraction of the sun. Moreover, there is one particular disturbing effect of the sun, known as the " parallactic inequality," which is proportional to the sun's distance from the earth. If this individual effect can be isolated from the others and accurately determined, the sun's distance can be calculated.

Velocity of Light. It would seem to be a far cry from the sun's distance to the velocity of light, yet in astronomy the two things find themselves closely related. Late in the seventeenth century Römer, a Danish astronomer, noticed that the eclipses of Jupiter's moons, as they disappeared from view into the shadow of that planet, occurred later when Jupiter was far away from us, and earlier when he was close to us. Römer saw that this was due to the fact that light must take a definite time to make the journey, and must therefore take longer when the intervening distance was greater. Since the paths of the earth and Jupiter are approximately concentric circles, the difference between the distances separating the two planets when they are on the *same* and when they are on *opposite* sides of the sun, is clearly equal to the diameter of the earth's path, or to twice the distance of the sun from the earth. Thus the number of minutes and seconds that the eclipses are delayed when Jupiter is furthest from us, as compared with the number when he is nearest to us, is actually the time taken by light to traverse the diameter of the earth's path. If the speed of light could have been independently measured, say over a given distance on the earth's surface, this tardiness of the Jovian eclipses would have given Römer the sun's distance. But in those days that was not possible, and Römer possessing already a rough knowledge of the sun's distance, used the eclipses for the opposite purpose of getting a rough idea of the velocity of light.

There is still another way in which astronomy links together the sun's distance and the velocity of light. Imagine yourself on a perfectly windless day standing still beneath your umbrella in the pouring rain. As there is no wind the rain-drops fall perfectly straight down, and in order to keep dry you hold your umbrella directly above your head. So soon as you start walking you will find it necessary to tilt your umbrella forwards to keep your trousers from getting wet: and the faster you walk or run the further forwards will you have to tilt your umbrella. The rain of course is falling straight down all the time; but as a result of your forward movement the direction of the rain-drops, *relative to yourself*, becomes slanted: the rain appears to be beating towards you, while the angle at which it falls towards you depends on the relation of your speed of walking to the velocity with which the rain-drops fall.

It is exactly the same with the light from the stars: the light is the rain, the movement of the earth corresponds with that of the pedestrian, and the telescope is the umbrella. If the earth were stationary, the telescope would have to be directed exactly to where the star really was. But since the earth is rushing through space, it has actually to be directed a little ahead of where the star really is. If the earth moved always in the same direction we should have no means of telling how far ahead of its true place we were looking when we saw a star; because, of course, we can only see the star where it appears to be. But since the earth moves round the sun in a big circle, the direction in which a star is apparently displaced also moves round in a circle (or an ellipse); and the centre of this circle is the true position of the star, the position in which it would be seen if the earth were stationary. Thus in the course of a year all the stars in the sky describe minute circular or elliptical paths, the half diameter of which, like the tilt of the umbrella, gives the ratio of the two velocities—which in this case is, how many times greater than the velocity of the earth the velocity of light is. Hence, if one could but measure the speed of light, the size of the stars' circles would give the speed of the earth in its path; and, since we know that this path takes precisely one year to be completed, we could straightway work out its length and diameter, the latter being twice the distance of the earth from the sun. This remarkable circular movement common to all the stars was discovered by Bradley, one of the Astronomers Royal of the eighteenth century, and is known as "aberration." Not knowing, however, the velocity of light Bradley could not use it in the way we have suggested; but it served as a valuable confirmation of Rõmer's discovery that light travelled with a definite speed.

Uncertainties in Sun's Distance. Since Cassini's seventeenth-century determination of eighty-seven million miles for the sun's distance, there had been others ranging as low as forty million. Early in the nineteenth century Encke worked out from the two transits of Venus (1761 and 1769) a distance of ninety-five million miles; and this figure was very closely confirmed by the French mathematician, Laplace, from a study of the moon's motions. But the matter was not yet to be settled, and about the middle of the century the lunar authorities demanded a reduction to ninety-one million to explain satisfactorily the perambulations of our satellite. In 1862 Mars again approached us closely, and as a result Simon Newcomb, the American astronomer, once more increased the distance to ninety-two million miles. It was at this juncture that the velocity of light at last succeeded in entering the arena. In 1849 and 1850 Fizeau and Foucault, in France, managed by independent methods to measure the time taken by a ray of light to travel to and fro over a certain distance between mirrors. By 1862 this difficult measurement had been sufficiently perfected to enter on an equal footing with the older methods in the solution of the astronomical problem; and the figure it gave was very close to the ninety-two million recently found by Newcomb. It was so obvious that this was the figure upon which all methods were tending to converge that the Nautical Almanac in 1870 made the change to ninety-two million from the value of ninety-five million which it had previously adopted as the basis of its calculations.

One might have thought that the whole matter could now have been allowed to drop; that the sun's distance was already known with sufficient accuracy; and that a million miles one way or the other was of purely academic interest. The sun's distance, however, enters as a fundamental factor into practically every other astronomical measurement and calculation. On it depend the movements of all the bodies in the solar system; and to convert the annual shifts in the apparent positions of the nearer stars into a measure of their distances, we must first know the distance of the earth from the sun. Our knowledge of the extent of the universe, our knowledge of the brightness, size and weight of each individual star, all depend ultimately upon our knowing the sun's distance. The sun's distance is the astronomer's " foot-rule," and no stone must be left unturned in finding out its precise length.

Transit of Venus. So it was that in 1874, at a cost of about a quarter of a million pounds, some fifty scientific expeditions were dispatched from all the chief countries in Europe and from America, to many distant parts of the earth, to observe the long expected transit of Venus. This large number of expeditions was desirable to eliminate as far as possible risks of bad weather, and to achieve greater accuracy by obtaining numerous independent results by diverse observers, instruments, and methods. And in order to get the maximum shift in the planet's apparent position, the stations had to be scattered as far north and as far south as practicable, which meant that some of them were in places outlandish in the extreme. There were expeditions throughout Russia, Siberia, China, and Japan; there were expeditions, some of them with a food supply for many months, upon many of the lonely islands in the Southern Seas. Over a year had been spent in preparation, and particularly in drilling the observers into flawless, almost automatic, working order.

And yet it all came to very little. When, on the great day, Venus made her appearance upon the face of the sun, she did not present the sharp black disc that was expected, but a diffuse and hazy outline, which defeated all attempts at precise measurement. This may have been due in part to the actual presence of a Venereal atmosphere, but was probably more the result of disturbances in our own atmosphere so often produced in the daytime by the heat of the sun. Although this event thus added little to our knowledge of the sun's distance, it at least taught astronomers that a transit of Venus was not after all an ideal method for attacking the problem.

Expedition to Ascension Island. Three years later, a very different type of expedition attacked the problem with a much more satisfactory result. In 1877 Sir David (then Dr.) Gill went to Ascension Island, near St. Helena, to observe the close approach of Mars. He adopted the principle, first used by Hipparchus for the moon, of sitting still and allowing the earth, by its rotation, to carry him, so to speak, from one side of it to the other. So, night after night, as he was carried to and fro over a distance of nearly 8,000 miles, he observed and measured the corresponding minute oscillation of the ruddy planet back and forth among the stars. Accompanied by Mrs. Gill, upon an isolated beach called Mars Bay, covered with a fine volcanic dust and infested with flies and mosquitoes, he carried on his observations for nearly five months. The results were definite and convincing, giving a distance for the sun of almost exactly ninety-three million miles. Thus where the concerted action of whole regiments of scientists had failed, one man taking up arms in single combat had succeeded.

But Gill did not leave the matter at that; there was, he felt, still room for improvement. It had been suggested that, instead of using the planet Mars for the purpose, one of the minor planets might be found to be more suitable. Since we left them in the last chapter, many more of these minute worlds had been discovered; and, though none was as yet known that came as close as Mars, their extreme minuteness made them pre-eminently suitable for exact measurement. In the telescope the minor planets appeared as simple points of light, just like stars, whereas, of course, Mars presented a disc of considerable size: and it is much easier to measure the position of a minute point of light than to measure the position of what one estimates as the centre of a disc. Already in 1874, in Mauritius, Lord Lindsay and Gill had tried the method out on the minor planet Juno; but, owing to a delay in the arrival of their yacht, the observations had been incomplete, and the results, though highly promising, could only be regarded as preliminary. In the years 1888 and 1889 Gill secured the co-operation of a number of observatories in making measurements of the minor planets Victoria, Iris and Sappho; and by 1897 he had succeeded in extracting from this mass of data a distance for the sun about 200 thousand miles shorter than that which he had found in Ascension Island.

The year after Gill had completed this arduous task a tiny body, only about fifteen miles in diameter, was added to the family of minor planets. It was called Eros, and was remarkable in that it occasionally approached nearer to the earth than any other body except the moon. In addition to possessing the advantage of minuteness, it thus out-rivalled even Mars in the matter of nearness. Its closest approach, to within sixteen million miles, about half the distance of Mars, had unfortunately occurred four years before its discovery, and would not happen again till 1931. However in 1901 it was due to come within thirty million miles of us and would undoubtedly give an opportunity for getting the sun's distance superior to any that had occurred before. In 1900 an international conference was summoned in Paris and a mass attack was planned by fifty-eight observatories; in 1901 Eros came and went; and in 1910 Mr. Hinks, who had undertaken the arduous job of investigating the results, produced a figure about 40,000 miles smaller than Gill's. That result is among the best we have to the present day; and, taken in conjunction with recent determinations, reached by methods depending on the velocity of light and on gravitational disturbances, we can say definitely that the sun is 92,870,000 miles away, and be sure that we are correct within about 20,000 miles.

Mars. And now, having allowed ourselves chronologically rather a free hand in our pursuit of the sun's distance, we must go back again to the year 1877. While Gill on Ascension Island was measuring night by night the tiny oscillation of Mars back and forth among the stars, another observer was scrutinizing the planet, just as assiduously, but with a very different purpose in view. Schiaparelli, the famous Italian astronomer, was taking advantage of the unusual proximity of Mars for making the first really accurate survey of its surface. For over two hundred years the planet had been studied with the telescope, but never before had it been subjected to so searching an examination. Schiaparelli's skill and thoroughness, the perfection of his instrument, the tranquil air of Milan through which he worked, and the closeness of the planet, all conspired to produce results of unrivalled excellence, which marked a new epoch in the study of Martian " geography." But there was another event which made this year a memorable one, and that was Schiaparelli's discovery of the so-called " canals."

Back in the seventeenth century, dusky markings had been noted on the surface of Mars with sufficient clearness to indicate their general permanence, and to show that the planet rotated on its axis slightly more slowly than the earth, in a period of about twenty-four hours thirty-seven minutes. In the eighteenth century two white spots had been detected at the planet's poles; and their seasonal waxing and waning had led the elder Herschel to conclude that, like the pole-caps in our own arctic and antarctic regions, they were composed of snow. Herschel had also observed transient white patches occasionally obscuring parts of the surface; and these he considered to be clouds, floating in a not inconsiderable Martian atmosphere. In the nineteenth century these conclusions had been generally substantiated; and the detailed knowledge of the planet's " geographical " features, after advancing with each instrumental improvement, had now as a result of Schiaparelli's survey taken a still longer stride forward. For many years the dusky markings had been regarded as *seas*, while the rosy background on which they stood was believed to represent dry land. When therefore Schiaparelli discerned a network of dusky streaks cutting across the rosy "continents," and connecting up the " seas " one with another, he naturally gave to them the name *canali* which, translated literally, simply means " channels." But somebody or other pounced upon this remarkable opportunity for sensationalism; and, by merely misconstruing the non-committal word, gave to it an implication which its originator was the last man ever to approve.

Now the story of the " canals " is a long and sad one, fraught with back-bitings and slanders; and many would have preferred that the whole theory of them had never been invented. Yet whatever harm was done was more than outweighed by the tremendous stimulus the theory gave to the study of Mars, and indirectly of the planets in general. Whether in a positive way to champion it, or in a negative way to oppose it, it attracted many able observers who otherwise might never have taken an interest in the planets. In particular it fired the enthusiasm of Percival Lowell, who in 1894 founded, for the purpose of studying the planets, and Mars in particular, the great observatory that bears his name. The Lowell Observatory was a unique institution. Never before had a large observatory been exclusively devoted to the planets; and Lowell, in founding it, spared no trouble or expense in insuring its success. Its site, on a high plateau in Arizona, was specially selected for the perfection of its atmospheric conditions; and its instrumental equipment is still among the finest in the world. Before this time the planetary surfaces had been left mainly to the care of the amateur astronomers, and though these were often highly skilled observers with powerful instruments, they naturally had at their disposal neither the means nor the leisure that a big institution could command. Thus the Lowell Observatory, with its ideal situation, its magnificent equipment, its elaborate organization and its capacity for long-continued, routine work, opened a new era in planetary astronomy. Since its foundation it has played a foremost part in this department of astronomy, especially in the development of new and important methods of investigation; and it is largely because of the valuable results it has achieved along these new lines, that other great observatories have been inveigled into sparing some of their precious nights for assisting in the elucidation of planetary problems. So the pistol which Schiaparelli had so unwittingly let off, though it shocked the finer feelings of many, had undoubtedly been the starting signal of that race for discovery which the planetary astronomers are still successfully pursuing.

The first important contribution of Lowell and his assistants was the discovery that the *seas* were not so homogeneously dusky as they had been supposed to be: the powerful telescopes revealed in them a mass of intricate detail quite out of keeping with the theory of water surfaces. The next step was to trace in these same seas variations in colour and intensity that were synchronous with the Martian seasons. If the dusky areas were indeed watery regions, it seemed that the emphasis in what we saw should be laid, not so much on the water, as on what was *growing in the water*. They should in fact presumably be regarded as the fertile regions of the planet, in contrast with the rosy background that represented desert. At this juncture, however, we must postpone till a later chapter further discussion about Mars, and pass in rapid review what was known at this time about the physical features of the other members of the solar system.

The Planets. Our short account of Mars has given us some idea of the sort of problem with which the planetary astronomer was mainly concerned. He studied the discs of the planets as revealed in his telescope to see what detail he could make out upon them. He was interested in the degree to which that detail was permanent or changing, in the physical interpretation he could put upon it, and in the period in which its movements indicated that the planet was rotating upon its axis. This kind of problem never cropped up in the case of the stars, because, being so infinitely more remote than the planets, they never looked bigger, even in the largest telescopes, than pin-points of light.

The planets were divisible into three groups according to their size and their distances from the sun. The four " terrestrial planets " lying nearest to the sun were Mercury, Venus, the Earth, and Mars—in that order from within outwards. The Earth was the largest, with a diameter of 8,000 miles, while Mercury was the smallest, with a diameter of just over 3,000 miles.

The " major planets " were the four planets Jupiter, Saturn, Uranus, and Neptune—Jupiter lying well outside the orbit of Mars, and Neptune being the most distant of all. These bodies were built on a much grander scale than the terrestrial planets, their diameters ranging from about 80,000 miles in the case of Jupiter to about 30,000 miles in the case of Uranus and of Neptune.

Finally there were the " minor planets," a vast conglomeration of small bodies, with diameters ranging from a few hundred miles down to only a mile or two, which were situated between the terrestrial and the major planets, in the gap between the orbits of Mars and Jupiter.

Mercury. The two terrestrial planets, which remain to be considered, Mercury and Venus, suffer from a common disadvantage. Lying as they do between the earth and the sun, they are never to be seen with the eye very far from the sunrise or sunset, where the atmosphere is always in a state of tremor and turmoil. Indeed, it was not until Schiaparelli adopted the method of studying them in full daylight, when he could observe them high up in the sky, that any results of value were obtained. In 1882 he began to observe Mercury in this way, and found that it had a spotty surface, which changed only by reason that the planet rotated on its axis. The rotation period seemed to be eighty-eight days, exactly equal to the period in which the planet revolved around the sun. Thus just as the Moon keeps always the same face towards the earth, so Mercury turns always the same side to the sun; and while about half of the planet's surface is scorched in eternal day—for there the sun is about eight times as hot as on earth—the rest of it is enveloped in eternal night. When Lowell started work on Mercury in 1896 he got results very similar to those of Schiaparelli; but whereas the latter thought he had found traces of a light Mercurial atmosphere, the former concluded that, if there was an atmosphere at all, it was entirely negligible. Thus Mercury from the point of view of habitability was completely out of the running.

Venus. The observation of Venus turned out to be considerably more difficult than that of Mercury. For two centuries vague shadings had been suspected interrupting the otherwise snowy whiteness of the " Evening (or Morning) Star," and the movements of them had been believed to indicate a rotation period of rather over twenty-three hours. There was no doubt that Venus possessed a thick and cloudy atmosphere through which we seldom if ever saw her actual surface. Now the rifts, or whatever they are, that are sometimes seen in, or through, these Venereal clouds, require for their discernment the most ideal conditions; and it is certain that the early records of their rapid movement were due to the planet being observed in twilight, low down near the horizon where our air is generally unsteady. In 1890 Schiaparelli was able to announce, as a result of his observations as far back as 1877, that Venus, like Mercury, kept always the same face to the sun, her periods of rotation and revolution being both equal to about 225 days.

Jupiter. Travelling outwards from Mars, the outermost of the terrestrial planets, and crossing the gap occupied by the multitudinous minor planets, we reach Jupiter, the nearest of the major planets. From the earliest days of the telescope, Jupiter's globe had been seen to be girdled about with dark belts roughly parallel to his equator. Cassini had long ago noticed that spots and other irregularities on these belts indicated a shorter rotation period for the equatorial than for the polar regions of the planet. In this respect Jupiter was similar to the sun; for sun-spots have a longer period of rotation the higher the solar latitude in which they are situated. This odd sort of rotation, his spinning round on his axis *not all in one piece*, can only mean that the visible layers are in a liquid or gaseous state. For many years it was believed that Jupiter was still in a molten condition, that, being so large a body, it had not yet cooled down, and was in an intermediate evolutionary stage between that of the sun and that of the solid terrestrial planets. The beautiful, almost glowing colours often seen in the planet's belts, the reds, browns, salmon-pinks and blues, lent weight to this theory, suggesting that through them one looked down upon the unextinguished remnants of the primaeval fires. There was, however, one thing which made it unlikely that Jupiter possessed any appreciable amount of self-luminosity. His four larger moons, discovered by Galileo, are continually passing between him and the sun; and, whenever they do so, they cast a round shadow upon his surface; and these shadows are, so far as one can see, *absolutely black.* It was thus felt that, though Jupiter was still in a molten state, his temperature was insufficient to produce any appreciable glow; and it was only a few years ago that cold water, and incidentally very cold water, began to be poured on this belief.

Saturn. We come next to the planet Saturn, the most distant of the five classical planets. The spectacle of Saturn and its rings is familiar to anyone who has opened a text-book on astronomy, or who has been fortunate enough to visit an observatory at a time when the planet was above the horizon. For Saturn is the super show-object in the heavens. Its remarkable appendages, the rings, are so far as we know unique in the universe. They were half discovered by Galileo, but their real significance proved too much for the imperfections of his telescope. It was some fifty years later, in 1655, that Huygens with his better telescope discerned their true configuration. The globe of Saturn, about 70,000 miles in diameter, is surrounded at its equator by a wide, flat but very shallow ring. The width of the ring is about 30,000 miles; the inner edge of it is separated from the planet's surface by about 20,000 miles; and yet the thickness of this stupendous structure probably does not exceed ten miles. The ring had been found by Cassini to be divided into two parts, separated from each other by a gap some 3,000 miles broad. This gap is called the " Cassini division " after its discoverer.

On the night of November 15th, 1850, William Bond, the first director of the Harvard College Observatory, detected with what was then one of the world's largest telescopes, a faint veil of light extending about half-way from the inner edge of the ring towards the globe of the planet. Two weeks later, before the news of the discovery had reached England, the same appearance had been noted by the Rev. W. R. Dawes, the " eagle-eyed Dawes," with the comparatively small telescope of his observatory at Wateringbury. This ring is known as the " crêpe ring," and is much fainter than the other two rings.

There had of course been much discussion as to what the rings were composed of. The original theory that they were solid structures, cart-wheels in fact, had gradually been displaced by one in which they were of a fluid consistence. In 1857 Clerk Maxwell, who is better known for his mathematical researches in electro-magnetism, investigated the problem, and showed that the only condition on which the architectural structure of the Saturnian system could avoid immediate collapse, was that the rings were composed of myriads of small particles, each revolving in its own individual orbit round Saturn. So the rings were really a vast multitude of tiny Saturnian moons, too minute and too closely packed to be seen individually in even the biggest telescopes. Now there is a principle known as " Roche's limit," discovered by Roche in 1848, according to which it is impossible for a moon to come nearer to its mother planet than a certain distance, without being shattered into fragments. And since the outer edge of the ring almost exactly marks this limiting distance in the case of Saturn, it seems likely that long ago a moon, coming within the danger zone, suffered the punishment of too close a familiarity with its parent.

This conclusion, which was, of course, purely theoretical, was confirmed by a most beautiful experiment nearly forty years later. When an express train dashes through the station the note of its whistle drops in pitch just as it passes one. As long as the train is approaching, the sound waves are crowded more closely together, like the folds of a closed concertina, and impinge on the ear in more rapid succession. As soon as it begins to recede the sound waves are drawn out further apart, like the folds of an open concertina, and impinge on the ear in less rapid succession. And as the pitch of a note is simply a measure of the frequency with which the sound waves hit the ear—the more frequently they arrive the higher the note—it is clear that an approaching whistle will have a higher note than a receding one. That exactly the same thing must happen with light waves, was first predicted by Doppler of Prague in 1842. For the colour of a ray is simply a measure of the frequency with which the individual waves reach the eye. But in the case of light, which travels enormously faster than sound, the effect is only appreciable when the source of light is approaching or receding with a prodigious velocity, the sort of velocity, in fact, found among the heavenly bodies. Even then, however, there is no actual change in colour. All the light rays of a rapidly approaching star are indeed shifted towards the blue end of the spectrum, or of a receding star towards the red; but the colour of the star will remain the same; for the yellow rays shifted into the green will be completely replaced by orange rays shifted into the yellow. It is rather like the old-fashioned type of escalator on which there were no individual steps, only a smooth, flexible strip travelling upwards over rollers. Watching it from a distance one could not tell that it had moved; for the position in the frame of each part as it moved was straightway taken by another part indistinguishable from the first moving up from below. But if one examines a receding or an approaching star with a spectroscope one can make out, as we explained above, dark lines in its spectrum indicating the presence of certain chemical substances in its atmosphere. Now these lines are like the steps in the modern type of escalator and enable one to detect whether any movement has occurred. They so to speak label the rays independently of their colour; so that one can tell at once from their position in the spectrum whether, for example, a line in the green really belongs to the green, or has been shifted into the green from the blue or yellow by reason of the movement of the star. Further back we said that the position of a spectral line was a characteristic of the chemical substance responsible for it. Clearly that statement must now be modified, since the position of the line is also affected by the motion of the source of light. How then can we identify the chemical element responsible for a given line, when we have no idea of the velocity with which the star is moving and thus cannot tell how far the line has been shifted from its normal position? Fortunately the shift in the line due to the motion of the source is small except when the velocity is unusually great; moreover, most chemical elements produce a group of lines rather than a single line, and this again enormously simplifies the process of identification.

The principle of the Doppler effect was first applied to astronomy by Huggins, who in 1868 started at Tulse Hill to measure the velocities with which some of the brighter stars were approaching or receding from us. In 1895 Keeler, director of the Allegheny Observatory in Pennsylvania, made use of this same principle as an experimental test of the satellite theory of Saturn's rings. If the rings were solid structures like wheels, their outer edges would have to be revolving faster than their inner edges, seeing that the former would have, in the same time, to describe larger circles. On the other hand, if the rings were composed of multitudes of tiny moons, each revolving in its own orbit, the velocity with which they revolved would be greater in proportion as they were nearer to the planet, being greatest close to the planet where its gravitational attraction was strongest. The experiment was indeed a delicate one, as the shift to be measured in the spectral lines was minute. But the difficulties were overcome, and Keeler demonstrated beyond doubt that the speed of revolution increased steadily from the outer to the inner parts of the rings, as could only happen if they were made up of myriads of discrete moons.

Coming now to the globe of the planet itself, we find that in general features it is very similar to that of Jupiter. Bright and dark belts girdle it about parallel to its equator. But whereas the belts of Jupiter are often grossly irregular in shape, bespattered with well-defined bright and dark spots, and continually undergoing change; those of Saturn are strikingly monotonous in their uniform tone, smooth contours, and lack of variation. Thus, while the rotation periods of the different parts of Jupiter can very readily be studied with accuracy, it is extremely rare to find on Saturn a spot or irregularity well enough defined to make use of for this purpose. Herschel in 1794 observed a spot which gave a rotation period of ten hours and sixteen minutes; but it was not until 1876 that a second opportunity arose, and Asaph Hall, of the United States Naval Observatory in Washington, discovered another spot which gave a period about a minute and a half shorter. In the early 'nineties other spots were studied in England, by Mr. Stanley Williams and Denning; and there was no doubt that, just like Jupiter, Saturn had a rotation period which varied slightly from one latitude to another. From this, together with its belted globe, one may assume that the physical condition of Saturn is very similar to that of Jupiter.

Uranus and Neptune. The planets Uranus and Neptune can be dismissed in a very few words, for up to the end of last century practically nothing was known about their physical features. Dusky belts not unlike those of Jupiter and Saturn had occasionally been suspected on both planets. One remarkable fact was the appearance in the spectrum of these two planets of a number of dark bands unidentified with any known substance. What was more, these bands could be faintly traced in the spectrum of Saturn, and still more faintly in that of Jupiter. Whatever their meaning was, they seemed to suggest some underlying physical similarity between these four outer worlds—some physical state, perhaps, which was least marked in the case of Jupiter and most marked in the case of Neptune.

The Minor Planets. We have already in the last chapter given some account of the minor planets. The first of them had been discovered on the first night of the nineteenth century, and by the end of the same century their number exceeded two hundred. The interest of the minor planets is of quite a different sort from that of the other planets; for, practically speaking, none of them presents an appreciable disc in the telescope. We have seen how some of them have been of great assistance in the measurement of the sun's distance. The problem of their origin—that of a family of tiny planets coming in the midst of a family of full-grown planets

—is in itself full of interest. To the celestial "treasure hunters," the astronomers who approach most closely the popular conception of them, they are a source of endless joy. For if one goes about it in the right way one may be certain sooner or later of discovering a few new ones. By the close of the century Max Wolf of Heidelberg had almost a hundred to his credit. On the other hand these minor planets are in a way rather a nuisance. It does not seem right to have innumerable little planets running about the solar system uncared for; having once discovered them, it seems incumbent upon astronomers to keep an eye on them. But this is an extremely arduous task: there is first of all the calculation of their orbits; and then, what is far more troublesome, the orbits must continually be revised, owing to the disturbing action of Jupiter, which is liable to produce considerable changes in the movements of these bodies. To cope with this difficult situation, a special bureau was founded in Berlin in the latter part of the century which carries on this complex and laborious work.

It is perhaps not very obvious why it is worth while going on discovering minor planets. It is of course true that now and again one is brought to light of peculiar interest in itself, as is well exemplified by the discovery of Eros in 1898. But there is another reason. The theory that these bodies resulted as a pure accident from the explosion of a single large planet, does not explain all the facts. It may well be that the origin of this strange progeny is a matter of much deeper significance in the evolution of our planetary system; and it is only through a thorough acquaintance with the number, distribution, and configurations of their numerous orbits, that we can hope to get to the bottom of the matter.

As regards the physical constitution of these bodies little was known. Their sizes could be roughly guessed from their brightness, by assuming a reasonable sort of figure for their capacity to reflect sunlight. Neison in 1880 estimated that the two hundred odd, discovered up to that time, would fill about one four-thousandth part of the volume occupied by the earth; while E. C. Pickering, at Harvard, from careful measures of the brightness of the individual bodies, got diameters ranging from 300 miles down to about a dozen miles. The first direct measures of size were made in 1894, when Barnard turned upon the four brightest of them the giant forty-inch lens recently installed at the Yerkes Observatory near Chicago. Ceres came out top with a diameter of 480 miles; Pallas, Vesta and Juno followed in that order with 300, 240 and 120 miles respectively. Though these four bodies appeared approximately spherical to Barnard, it is quite probable that some of the smaller of them, only a few miles across, are irregularly shaped masses of bare rock. This at least seemed to be so in the case of the tiny Eros; for, in 1901, von Oppolzer found this speck of light to be indulging in large and rapid fluctuations in brightness. About every two and a half hours its light would fall off to a quarter of its normal intensity; and, presumably, that was the period in which it rotated upon, or rather tumbled about, its axis.[1]

Satellites. In 1866 great excitement was aroused by the announcement from Schmidt of Athens that a small crater on the moon, called Linné, had disappeared. Changes on the moon had been reported before but never substantiated; and it was generally agreed that they did not occur. But Schmidt was one of the greatest authorities on the moon; and it was a long time before this latest sensation subsided. Even now, there is some doubt whether or not any change took place; for visual impressions and free-hand drawings were the only evidence available. Yet the occurrence was an important stimulus to lunar observation, and specially to the development of lunar photography, which at that time was still in its infancy. The systematic study of the moon's surface had started at the end of the previous century with the work of the energetic, but over-imaginative, Schröter, of Lilienthal near Bremen. Nevertheless he had at least laid down the tactics for the future campaign: he made numerous drawings of the moon's surface, he classified the various types of lunar formations, he searched for signs of an atmosphere, and he looked for evidence of change in the landscape. The whole subject, however, was given much greater solidity by the trigonometrical survey subsequently undertaken by Beer and Mädler in Berlin. In 1837 they published their results in the form of a map thirty-seven inches in diameter, giving a scale at the centre of nearly an inch to fifty miles, together with their measures of the heights of 1,100 of the loftiest mountains. Other maps followed, culminating in Schmidt's publication, in 1878, of a map over six feet in diameter showing the positions of nearly 33,000 craters.

The moon's surface, rugged and mountainous in the extreme, was generally held to be the result, in a past age, of the wrinkling of its surface during the rapid cooling of its comparatively small bulk; for the moon is only 2,000 miles in diameter and has but one-eightieth of the earth's weight. The numerous ringed plains, the so-called lunar craters, which are scattered widely over the moon's surface were regarded, as their name implies, to be of volcanic origin.

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[1] It now appears that the rotation period is twice as long as that found by Oppolzer, the planet having two light-maxima during each rotation.

That the moon has an excessively rare atmosphere, if indeed it has one at all, is a matter of simple observation. The shadows on its landscape are dead black and extremely sharp as though cut with a razor. There are no half-shadows, no twilight; where the sun is setting one passes directly from full daylight to darkest night. When we watch the stars disappear behind the moon they go out " pop "—there is no vestige of fading, or of lingering on its edge, as there would be if there were an atmosphere. In 1897 Professor Comstock went into this matter thoroughly with the 15-inch lens of the Washburn Observatory, Wisconsin. He could detect no lingering of the stars, and he determined the amount of lingering he could have detected had it been there. He found that, if the moon had possessed an atmosphere onefive-thousandth part of the density of our own, he would have detected a hesitancy in the disappearing star. So the moon's atmosphere was either non-existent or negligible—a result quite in accord with the theory given thirty years before by Johnstone Stoney: that, since the gases of an atmosphere are always tending to escape, light bodies like the moon have not the gravitational power to retain them.

But probably the outstanding event of the period in this branch of astronomy was the introduction at last, after so many attempts, of the photographic plate for the delineation and charting of the lunar surface. The greater part of that which took Schmidt thirty-four years of continuous work to achieve, could now be accomplished by a few momentary exposures spread over a month or two. Of course the photographic plate, especially at that date, missed much of the delicate detail caught by the eye; but that was more than compensated for, for most purposes, by the vastly superior accuracy of the impersonal record. In 1896 there appeared independently the " Part One " of two photographic atlases of the moon, one emanating from the Lick Observatory in California, the other from Paris. Apart from their value as maps, and apart from their value in the study of lunar formations, these atlases gave a new feeling of confidence: should ever the volcanic forces pent up inside the moon—if indeed they had not long ago subsided—stir again its desolate wastes, the event, unless trivial, could scarcely pass unnoticed.

Although the possession of satellites is a common trait among planets, we on the earth have at least the distinction of owning *for* *our size* the largest of them—a matter of special interest as we shall see later. Mercury and Venus are both barren; and this same state was enjoyed by Mars until 1877 when Asaph Hall at Washington presented him with two minute attendants. That was indeed an important year for Mars: Gill's expedition to Ascension Island, Schiaparelli's survey and discovery of the canals, and finally the detection of his two satellites. Phobos and Deimos, as they were called, were tiny bodies each about ten miles in diameter. Phobos was unique, for it revolved round Mars in a third of the time it took Mars to rotate upon its axis. So Mars had three Phobian months per day. Oddly enough, Dean Swift, a hundred and fifty years before, had made the Laputan astronomers in his *Gulliver's Travels* discover two Martian moons, one of which went round the planet two and a half times in a Martian day—and yet this is the only known example of a satellite revolving in a shorter time than its parent planet rotates.

The four great satellites of Jupiter were discovered so soon as Galileo turned his telescope upon the planet. Although all but one of them are larger than our moon, they are very small in comparison with the huge mass of Jupiter. In 1892 Barnard with the 36-inch lens of the Lick Observatory added a new member to the family. The fifth satellite revolves close to the planet and is so insignificant in size that no one has troubled to give it a name.

In addition to the myriads of tiny moons that compose its rings, Saturn possesses a considerable family of ordinary satellites. The names of the seven, known up to the end of the eighteenth century, were given from without inwards in the following pentameter and half:

" Iapetus, Titan, Rhea, Dione, Tethys,
Enceladus, Mimas."

In 1848 there occurred one of those duplicate discoveries which are of curiously common occurrence. On September 18 William Lassell with his two-foot mirror near Liverpool detected between Iapetus and Titan an eighth satellite; and on the following night Bond (the discoverer of Saturn's " crêpe ring ") independently discovered the same object at Harvard. It was called Hyperion; and was followed in 1898 by Professor W. H. Pickering's discovery of a ninth, yet fainter and very distant satellite, called Phoebe, which exhibits the remarkable behaviour of revolving backwards in the opposite direction to that of all the other satellites.

William Lassell distinguished himself by discovering no fewer than four out of the eight satellites found during the nineteenth century. By trade he was a brewer, but at a very early age he took up astronomy as a hobby. At the age of twenty-one, having not much money, he emulated Herschel and started making his own telescopes. He ultimately produced an instrument 36½ *feet long with a mirror 4 feet in* diameter which was at the time *second only in size to* Lord Rosse's telescope. An earlier telescope of his, 2 feet in diameter was completed in 1846, and in that same year, within three weeks of the discovery of Neptune, he made his first satellite discovery by detecting the solitary and nameless attendant of that planet.

The other planet upon which Lassell bestowed much of his attention was Uranus. Herschel, a few years after his discovery of that planet, had found two Uranian satellites, which are now called Oberon and Titania; and though subsequently he suspected four others he could never make certain of them. Lassell, therefore, spent many years in searching for these hypothetical bodies; and it was not until 1851 that he finally discovered Ariel and Umbriel which, it is practically certain, Herschel could never have seen.

We must postpone to a later chapter the interesting question of the origin of our moon and of the other moons in the solar system. Here we shall simply refer to the invaluable service which satellites in general render to astronomers: their provision, for their respective planets, of a celestial weighing machine of the most delicate precision. Since according to gravitational theory the period in which one body revolves round another depends only on the combined weight of the two of them and on the distance between them, we have merely to measure the period of revolution and the distance separating them to be able to calculate their combined weights. And since by good fortune the satellites, with the exception of our own, are minute in comparison with their parent planets, the combined weight of the two is, almost exactly, the same as the weight of the planet alone. Hence, whereas all the planets which possess satellites have been weighed with high precision, their barren brother and sister, Mercury and Venus, have had to be weighed with a much less delicate pair of scales. Venus is weighed by her disturbing effect on the motions of the Earth and Mars, while Mercury is weighed by his disturbing effect on the motion of Venus. Consequently, there is an uncertainty in the weight of Venus amounting to nearly one per cent., and in the weight of Mercury of close on twenty-five per cent.

CHAPTER IV

THE BEGINNINGS OF CELESTIAL PHOTOGRAPHY AND DEVELOPMENTS IN TELESCOPE CONSTRUCTION

Celestial Photography. Without a doubt the most significant development during the middle part of last century was that of astronomical photography. Probably the photographic plate ranks second only in importance to the telescope as a weapon of astronomical research. Even the spectroscope, the wonderful revelations of which we began to disclose in the last chapter, could not have carried us very far without the assistance of the camera.

In 1839 Daguerre and Fox Talbot had almost simultaneously invented the two earliest methods of obtaining permanent photographic pictures. In 1845 Fizeau and Foucault made in Paris a not altogether unpromising daguerreotype of the sun. But the first achievement of real note was a daguerreotype of the moon taken by Bond at Harvard in 1850; while in the same year at the same observatory the much feebler radiance of the star Vega was prevailed upon to record its mark—for it was scarcely anything more—in a similar manner. Bond's picture of the moon had been exhibited in London at the 1851 Exhibition, and was the direct cause of Warren de la Rue, the English amateur astronomer, deciding to devote his energy to astronomical photography. De la Rue hailed from Guernsey and was a wealthy paper manufacturer in this country. In 1853 he was the first to apply to astronomical photography the wet-plate process invented by Archer two years before; and by its means he got some extremely successful lunar photographs. He designed for the Kew Observatory a special camera for photographing the sun and making records of sun-spots. This he provided with an ingenious type of shutter; for it was necessary to give exposures of almost infinitesimal duration in portraying so dazzling an object. Thus started the daily photographic record of the state of the sun's surface; it was carried on at Kew from 1858 to 1872, and was then transferred to the Royal Observatory, Greenwich, where it has continued ever since.

Solar Photography. This event, the institution of routine solar photography at Kew, was pregnant with significance. It foreshadowed, or rather was the first step in, that great movement to replace, in one branch after another of astronomical research, the eye by the photographic plate. The method of photography had enormous potentialities as an astronomical labour-saving device; moreover, the record was impersonal and permanent, and, within certain limits, accurate and complete. These points were well illustrated in the routine photography of sun-spots. The record was obtained in a small fraction of the time required to make the visual observations; and on a cloudy day the sun's momentary appearance in a gap would be sufficient to complete the day's work. The Kew observers were mainly interested in the number, positions and sizes of the spots in relation to the study of the sun-spot cycle; and all these data could be determined from the plates at their leisure and convenience; but, what was more important, should they on a subsequent occasion require to investigate some other characteristic of the spots, they had simply to refer back to the plates. So far, however, the plates were only feebly sensitive to light, and thus the sun, alone among the heavenly bodies, was suited for routine photographic study. Even the light of the moon was too pale to produce anything more than small scale pictures; and these, though interesting as experiments, showed no detail to compare with that revealed in the direct telescopic view.

Eclipse Photography. But the branch of astronomy which more than any other required the assistance which the photographic plate could give was the observation of total eclipses of the sun. Without the aid of photography the delineation of the complex structure of the corona and prominences for subsequent study could never be accurately performed in the brief moments available during totality; still less could one hope to learn much of the spectrum of these appendages, when the first step in the problem was necessarily the exact measurement of the positions of numerous spectral lines. De la Rue was of this opinion, and in 1860 he and Secchi took with them to the Spanish eclipse two telescopic cameras. We saw in the last chapter how the immediate result of their enterprise was to settle once and for all that the prominences emanated from the sun and not from the moon. Henceforward, the plate practically replaced the eye in the *direct* study of the corona. But in the study of the spectrum of the solar appendages the eye had still for many years to do its best; for no plate was sensitive enough to record the feeble rays that suffered so much impoverishment in their passage through the spectroscope. It was not until the 1882 eclipse, just after the invention of the much superior dry-plate process, that the camera was successfully wedded for the first time to the spectroscope. This was the beginning of its final triumph in the eclipse camp; and in the succeeding years its evidence came more and more exclusively to be relied on. In fact, by the end of the century it was a not uncommon complaint among astronomers, that though they had been present at several of these much talked-of displays, they had never had an opportunity for looking at the corona, so busy were they in operating their cameras.

Photography of Stellar Spectra. The invention of the dry plate was a tremendous boon to Huggins, who for more than a dozen years had tried in vain at his observatory in Tulse Hill to obtain photographic impressions of stellar spectra. As he pointed out, the complete study of the spectrum of a *single* star was, by the visual method, the work of many years. If one could but get a good photographic record, the measurement and identification of the lines on the plate could be carried out much more conveniently and quickly. Yet in the visual method Huggins had made great strides, and, in two or three of the stars, he had measured and identified with those of known substances some seventy of the celestial radiations.

In 1876 Huggins exposed for the first time a dry plate to an extra-terrestrial source, and obtained a promising picture of the spectrum of the bright star Vega. By 1882 he had so perfected the technique that he had succeeded in portraying the spectra of many of the brighter stars, of the comet of 1881, and, most difficult of all, of the faint filamentous clouds of the Orion Nebula. But not only did the photographic method mitigate the labour and augment the accuracy of his observations; it also extended them into an unexplored region. For the photographic plate is able to discern the invisible radiations which lie beyond the violet end of the spectrum. Henceforward, the camera was the weapon of choice for Huggins and for his recently acquired and devoted assistant, his wife.

Prismatic Camera. It was at this juncture that America, always appreciative of the principle of mass-production, stepped in with a method for photographing stellar spectra wholesale. In ordinary spectroscopy one uses a *slit*: the light source to be examined illuminates the slit, and the spectral band that is seen is actually a drawn-out image of the slit; while the dark lines are gaps in the spectrum representing the absorption from it of rays of particular colours. But in the case of the stars the slit is not always necessary. If one puts a prism in *front* of a camera and turns it on the stars, the image of each star will appear drawn out into a line interrupted here and there by dark gaps. The line is the spectrum, and the gaps represent the spectral lines. Of course, a line with gaps is difficult to measure; but, by giving the camera a suitable movement by clockwork in a direction at right angles to the linear spectrum, it can obviously be drawn out into a band on which the gaps will now appear as lines—just like a spectrum produced in the ordinary way with a slit. The shape of a spectral " line " is the same as that of the slit, and if there is no slit its shape will be the same as that of the source. Thus, if one tried to get the spectrum of the sun or moon without a slit, the " lines " would be large round discs hopelessly overlapping and confused. By dispensing with a slit in the case of the stars there are two advantages. First, less light is lost and one can photograph the spectra of much fainter stars than is possible with the use of a slit; secondly, one gets with a single exposure the spectral record, not only of one star, but of as many stars as are covered by the field of view of the camera, dozens, perhaps hundreds, at a time. Although the slitless spectroscope, or " prismatic camera " as it is called, has these two advantages over the slit-spectroscope, it has certain disadvantages. When making measures for the identification of stellar lines it is necessary to introduce into the slit of the instrument, simultaneously with the light of the star, the light from an artificial source, and to photograph both spectra accurately alongside each other on the plate. The terrestrial and celestial lines can then be compared and identified with each other very accurately. But this is impossible unless a slit is employed. Thus while the slit-spectroscope must always be used in work of high precision, the enormously more rapid turnover of the prismatic camera makes it in many cases the instrument of preference.

We saw that the first steps in the classification of stellar spectra had been taken by Father Secchi, who from a survey of the spectra of the brighter stars found it posible to divide them into four groups. Vogel of Potsdam had followed with a similar survey which, though it was confined to a more limited region of the sky, extended to fainter stars. To his classification, which differed only in detail from that of the Jesuit astronomer, he had added a physical interpretation. The hot, white stars, rich in hydrogen, were considered to be in the heyday of their youth; the cooler yellow stars, like our sun, whose atmospheres abounded in metallic vapours, were of middle age; and, lastly, the red stars, whose beautiful fluted spectra indicated a comparatively low temperature, were presumably in the evening of their life-time, the final stage before extinction.

This much had been achieved by visual methods, as a result of the laborious work of many years. And then in 1886 the Harvard Observatory, with E. C. Pickering at its head, undertook to make over the entire heavens a photographic survey of the spectra of all stars down to, and even beyond, the limits of the naked eye. The project would have been impracticable but for the development at Harvard of the wholesale method of the prismatic camera. As it was, the survey of considerably more than the entire northern hemisphere was completed, and published, within four years. In fact, by 1889 the instrument had been transferred to Peru and had started work on the southern hemisphere!

The Draper Memorial Catalogue, for the undertaking was in memory of Henry Draper, of New York, whose pioneering work in stellar spectroscopy had been terminated by his early death, had two main objects in view. First, it set out to create a readily workable and precisely defined spectral classification; and secondly, by extending its classification over the entire sky to all stars down to a certain limit of brightness, it aimed at providing a perfectly homogeneous mass of data which would be suitable for statistical investigation: for, as Sir William Herschel long ago foresaw but America first fully realized the statistical method was our one ground for optimism in the attempt to unravel the puzzle of the universe.

Actually, the Draper Memorial Catalogue achieved much more. First it achieved, or *is* achieving, what appears to be a measure of immortality; for, instead of coming to an end when its first object was accomplished, it has gone on extending its limits and enlarging its scope, a " living work " in the truest sense. In the second place, during the course of its production, many by-products of value emerged.

The first result of this sort was in the classification itself. Not only was the classification produced and defined, built upon the foundations laid down by Secchi and Vogel, but it was found possible so to arrange it that the vast majority of the stars could be placed in a perfectly orderly and continuous sequence, in which each type merged by imperceptible gradations into the next. Even the small minority left over could be placed in subsidiary sequences, traceable as alternative branches from the main sequence. It seemed certain that such orderliness in the celestial types must represent some form of evolutionary scheme. But whether or not it was of the kind suggested by Vogel was quite a different question. It was possible that, what appeared to be a one-way, was in reality a two-way street. In 1887 Lockyer had propounded a theory of stellar evolution in which the stars not only died red as with Vogel, but also were born red. Condensing out of the primeval nebula they climbed the scale from red to white heat, whence they declined in the opposite direction through yellowness and redness to death. This much of the theory was strikingly prophetic of modern ideas; but the conceptions underlying it went wider of the truth. Lockyer thought that at the terrific temperatures of the hottest stars the heavier chemical atoms were broken down into atoms of hydrogen and helium. In this way he sought to explain why the spectra of the cool red stars revealed the presence of the majority of the chemical atoms, while those of the hottest white stars showed absorption almost exclusively by hydrogen and helium. So, perhaps, after all, the alchemist's dream was to be realized, far away in the dying embers of the celestial furnaces. The theory was by no means universally accepted; and Huggins, for one, inclined to the view that the different types of spectra represented, not so much differences in the composition of the stellar atmospheres, as differences in the ability of the various gases to absorb light under the varying conditions of temperature.

Velocities of the Stars. The year 1887 held another triumph in store for spectral photography. We have already explained the Doppler effect: how it is responsible for the familiar drop in the note of a receding train-whistle, and likewise, in the realm of optics, how it produces a shift in the spectral lines of a source of light that is rapidly approaching or receding. We anticipated our story by telling of Keeler's use of this same principle in 1895 to measure the rotation of Saturn's rings. Huggins, so often before a pioneer, was the first to apply the principle in 1868 to the stars. He found the dazzling Vega to be receding from the earth at about thirty miles a second, and, six years later, was able to announce the velocities towards, or away from, the earth of no less than thirty of the brightest stars. But the shifts in the spectral lines that he essayed to measure were so minute as to be well-nigh on the limit of what his eye could detect. Thus, his actual figures were subject to grave uncertainty. Now the photographic plate has a remarkable but familiar property: the longer it looks the more it sees. If Huggins had rolled out his star spectra to a greater length and thus enlarged the tiny shifts he was trying to measure—as he could have done by passing the starlight through a more powerful spectroscope—he would have so enfeebled the brightness of the spectrum as to make the measurement impracticable on that score. But with the photographic plate it was different : it was possible to make up for the loss of brightness by giving a longer exposure. The credit for taking this step, of using the photographic plate for measuring the spectroscopic velocities of the stars, is due to Vogel. Between 1887 and 1892, by employing powerful slit-spectroscopes, rapid dry-plates, and long exposures, he determined the velocities towards, and away from, the earth of over fifty stars, with an accuracy far surpassing that of the visual measurements. In recognition of the surprising success of the new method, and with a view to making it available for much fainter stars, the existing 11-inch telescope at Potsdam was superseded in 1900 by a magnificent telescope with a 30-inch lens.

Double Stars found by Spectroscope. In 1889 a new sort of celestial object was found. Its discovery constituted one of the first and most important achievements of the Draper Memorial Catalogue. The plates exposed during the survey had been arranged to overlap one another, so that every star was recorded on three or four occasions. While they were being examined, a small star in the tail of the Great Bear was found to have a strangely variable spectrum. In two of the pictures all its lines were seen to be double, in two others they had the normal single appearance. More photographs were taken, and it was found that the tiny spectrum went through its changes rhythmically, the lines dividing into two and separating, then closing up and coalescing, in a perfectly regular period. The only possible explanation was that this star, which looked single in the largest telescope, was actually a pair of suns in close and rapid revolution round their centre of gravity. At one time they would both be moving at right angles to the line joining them to the earth, and then, as neither of them would be moving towards or away from the earth, their two spectra would be exactly superposed. At other times their motions would be directed more or less towards or away from the earth, and since one of them would always be approaching while the other was receding, their spectral lines would be separated, being shifted in opposite directions. This was the first of the spectroscopic double stars to be discovered; but, as soon as they had attracted attention, others were sought and discovered in large numbers all over the sky. They naturally fell into line with the visual double stars which William Herschel, long ago, had by direct measurement shown to be slowly revolving.

Direct Stellar Photography. The most spectacular achievement of photography was in its dealings with the nebulae. Everyone is familiar with the magnificent pictures of these objects to be seen in astronomical books. The great strength of photography lay in its ability to see more and more the more it looked. With *point-like* objects such as stars, the bigger the telescope the brighter they appear; but with *extended* objects like the nebulae this is not so. True, the bigger telescope collects more light from them in the first place; but since it also magnifies them more, the larger amount of light is merely distributed over a correspondingly larger image, so that each little bit of the image appears no brighter. But the photographic process with its ability to accumulate the luminiferous effect, can go on intensifying the picture on the plate indefinitely.

In this field Dr. Common at Ealing, with his home-made reflecting telescope, three feet in diameter, made important advances, and in 1883 got the first really successful nebular photographs. His beautiful pictures of the Orion nebula, which won him the Gold Medal of the Royal Astronomical Society, not only revealed in its depths detail never before suspected, but showed its filamentous clouds to extend far out beyond its previously supposed limits. Later, with a 20-inch mirror at Crowborough, Isaac Roberts obtained pictures of certain nebulae revealing, in sharply cut contours, that spiral structure which the eye had only faintly traced in Lord Rosse's giant telescope. But the best results came in 1899 from a series of pictures taken at the Lick Observatory by Keeler. The instrument was Common's 3-foot mirror which had recently come into the hands of that institution; but it was now given the double advantage of the clear air of the Californian mountains and of the more sensitive modern plate. The photographs revealed multitudes of small nebulae hitherto unknown, often over a hundred on a single plate. A conservative estimate put the total number of these objects within reach of the instrument at more than a hundred thousand. The strangest revelation of all was that the great majority of them showed the mysterious spiral configuration. What was the significance of these multitudes of celestial whirlpools, nobody at the time could even guess.

So far the photographs we have described had been taken with *telescopes*, mirrors or lenses, which portrayed on a comparatively large scale a very small region of the sky. But another type of instrument was coming into use, the *astronomical camera*, like an ordinary camera but bigger, which portrayed on a comparatively small scale a much larger region of the sky. It was an instrument of this sort with which in 1889 Barnard, at the Lick Observatory, began to photograph the Milky Way with its enormously extensive star-clouds. First, with a camera lens six inches in diameter, and later, with one ten inches in diameter, he gave exposures of many hours, and displayed more effectively than ever before the almost appalling grandeur of the sidereal universe. Banks upon banks of stars, every one at least the equal of our sun, were piled up on each other, so that in many places they seemed to coalesce to form a solid wall of light. Here and there, clear cut, were extraordinary black holes and straggling black lanes completely empty of stars, like tunnels and crevices penetrating the universe. And then again, often in close proximity to these vacancies, were filaments of nebulous haze woven in and out among the star masses like delicate lace. There was no doubt that, in the direction of the Milky Way, we were looking right through the *length and breadth* of our stellar system, which, like the rings of Saturn, possessed a *depth* by comparison far less extensive. And yet, that being so, how could those tunnels and crevices by pure accident be so miraculously straight and so precisely directed towards us? As Barnard first pointed out such an explanation was very improbable; and what appeared as holes in the universe must, in reality, be dark celestial clouds blotting out the vistas beyond. Thus he introduced the idea of dark, obscuring nebulae—some variant, presumably, of the luminous nebulae—and sounded the alarm for the existence of what has become, as we shall see later, one of the greatest bugbears of the astronomer.

In 1891 Max Wolf at Heidelberg made the first photographic discovery of a minor planet, and in the following year Barnard discovered the first new comet by the same method. One cannot recognize a minor planet by simply looking at it; for in appearance it is indistinguishable from a star: one has to watch it for at least half an hour, when its movement among the stars will betray its true nature. Comets, however small and faint, have usually a slightly fuzzy appearance; but again one can only with certainty distinguish them from nebulae by watching for their movement. The photographic plate has the enormous advantage that it records the movements of members of the solar system automatically. During an exposure of about half an hour, most of such bodies will have moved appreciably among the stars. Thus, whereas the stars appear as dots, a minor planet or comet will betray its presence by showing a definite "trail" on the plate. Since Wolf and Barnard first set the fashion in this matter, most new objects of this sort are discovered by the photographer:

in fact, as regards minor planets nobody in their senses would now-a-days dream of looking for them in any other way.

And now the strongest citadel of visual observation began to topple. The distances of the stars, as we saw in the last chapter, required for their detection the most exacting precision in measurement. It seemed almost absurd to suppose that such extreme accuracy could be entrusted to a film of gelatine spread on glass; especially as, after the images had been imprinted thereon, it had to be developed, washed and dried before the measurements could be made. Yet the possibility of shrinkage and distortion during development was only one of many likely dangers. Nevertheless it was out of Oxford, that reputed stronghold of conservatism, that this heresy sprang. It was the suggestion of Pritchard, the Savilian Professor of Astronomy; and between 1886 and 1892 he determined photographically the distances of some thirty stars. But his technique was poor, and it was left for the illustrious Kapteyn, of Groningen, to put the new method on a solid foundation and give in 1900 his preliminary results for 250 stars. It turned out that photography was far superior to visual observation for this purpose, and now the distances of stars are always measured photographically.

Finally we come to the greatest achievement of all—probably the biggest undertaking ever set on foot by astronomers. It all arose from His Majesty's Astronomer at the Cape borrowing a hand-camera from a local photographer! The Cape Observatory possessed no photographic equipment; and when in 1882 one of the brightest comets of the century suddenly blazed forth, Gill thought it worth while trying to get a picture by the only method available to him. Of course the little camera had to be tied on to the telescope tube, in order that it might be kept moving during the exposure, to counteract the earth's rotation. But as far as the photograph was concerned the hand-camera did all the work unaided. The pictures obtained were of great beauty, by reason of the splendour of the comet; but the point that impressed Gill was the multitudes of stars they incidentally showed. Was not the camera, he asked himself, the ideal instrument for charting the heavens?

The great atlas and catalogue of the northern hemisphere, Argelander's *Bonner Durchmusterung* published in 1863, gave charts and positions for over 300,000 stars; but the greater part of the southern hemisphere remained unmapped. Gill, therefore, decided to try to fill the gap photographically. We have just discussed the difference between the photographic telescope and the astronomical camera. The instrument which Gill now ordered from Europe was of this latter type, and it was probably the first of its sort to be used. The camera, which had a lens six inches in diameter, was 4½ feet long. The plates, each taken in duplicate, were completed in six years; but it was not until 1900 that the catalogue, giving the positions of over 400,000 stars measured by Kapteyn, was published. The accuracy and completeness of the catalogue and charts far surpassed that of the great visual work which it had modestly set out to extend.

In 1886, only a year after its commencement, the extraordinary success assured to the undertaking was so abundantly manifest, that Gill formally proposed an international organization for charting the entire sky by the same method, though much more accurately and on a much larger scale. It happened that in the meantime the brothers Paul and Prosper Henry of the Paris Observatory had designed and made a photographic instrument—something half-way between a camera and a telescope—for charting a certain part of the Milky Way, where the number of the stars had completely baffled all attempts at visual measurement. Here again the results excelled all expectation; and the proposal for an international congress, heavily backed from both sides of the Channel, brought to Paris in 1887 no less than fifty-six representatives from seventeen different nations.

It was decided that the work on the Carte du Ciel or Astrographic Chart as it was called, should be shared out between eighteen different observatories, each equipped with a standard instrument of the Henrys' design, a camera 11¼ feet long, with a lens 13 inches in diameter. The Astrographic Chart and Catalogue, will comprise when completed about 11,000 charts, and a catalogue of several hundred volumes giving the positions with a very high degree of accuracy of between three and four million stars. Only now, at the time of writing, is this vast undertaking, set on foot nearly half a century ago, approaching its completion. The purpose of it has been to lay down solid foundations, in the form of exact positions of all stars down to a brightness several hundred times fainter than can be seen with the naked eye, for the future study of stellar motions. Our knowledge of the structure, dimensions, mechanism, and evolution of the universe largely depends on an exact knowledge of the movements of the bodies composing it. The velocities of these bodies in a direction towards or away from us we can measure with the spectroscope. Yet that is only half the question: it still remains to determine their velocities at right angles to that direction. Such velocities can only be ascertained from measurements of the positions of the stars in the sky. What is required is a series of precise measurements, followed after an interval—and the longer the interval the better—by a second series of equally precise measurements. That is the reason why already, even before the Astrographic Chart has been completed, astronomers have started to repeat the whole process over again.

Telescopes. Before closing the present chapter we must turn to consider the progress which had been made in telescope construction during the second half of the nineteenth century. Since the erection of Lord Rosse's great reflector in 1845, there had been notable advances; for during the second half of the century the refractor more than doubled its size and an entirely new principle was discovered in the construction of reflectors.

The metal mirror of Lord Rosse's telescope was no less than six feet in diameter; yet in 1847, two years after its erection, the two largest refractors in the world were only fifteen inches in diameter, while those of which the diameter exceeded nine inches could have been numbered on one's fingers. The glass for all these instruments had come from Germany, Switzerland or France, while the lenses had been made without exception by the opticians of Munich or Paris. One of the 15-inch telescopes was, as we have already seen, at Pulkowa; the other which was one of the first big telescopes to go to America was at the Harvard College Observatory. In our own country there was the 11½-inch at Cambridge with which Challis had conducted his leisurely search for the planet Neptune, a 13-inch telescope at the private observatory of Edward Cooper of Markree Castle, Ireland, and the 12-inch telescope purchased as early as 1830 by Sir James South. This last instrument was at one time the largest refractor in existence, but its owner in a fit of temper had smashed its mounting to pieces and it was not until years later, when the lens was presented to Dublin University and remounted, that it began to be put to good use. Lastly, in the same year that Harvard purchased its 15-inch refractor, the citizens of Cincinnati procured from the same firm of Merz a fine 11-inch refractor.

These were, in 1847, the chief large refracting telescopes in the world; and during the next fifteen years it seemed as though the diameter of 15 inches achieved by the German opticians was a limit unlikely to be passed. The next advance came from a most unexpected quarter. It was only recently that the Americans had begun to take an interest in astronomy. Their enthusiasm had been fired by the apparition of the great comet of 1843, and immediately afterwards there came into existence the first three American Observatories, the one at Cincinnati, the Harvard College Observatory, and the United States Naval Observatory at Washington. But America was not for long to remain content to purchase telescopes from the European market; instead, within a few years it was to lead the world in telescope construction.

In Cambridgeport, in Massachusetts, a portrait-painter, one Alvan Clark, began to amuse himself with experiments in grinding and polishing lenses. Soon his skill gained fame for him, and he was commissioned to make a large lens for the University of Mississippi. This lens with the unheard-of diameter of 18½ inches was completed in 1862. While in the process of testing it its maker discovered that tiny star—the Companion of Sirius—the existence of which had been predicted by Bessel eighteen years before.

The next step forward was taken in England. It seems strange that though the achromatic telescope was first invented in this country by Hall about 1733, and independently by Dollond in 1758, there was no noteworthy contribution to its subsequent development by an English optician for over a hundred years. One reason, of course, was that the art of glass-making only reached England in 1848. Yet at an earlier date, when France was still uninitiated in the art, the French opticians had produced some of the finest lenses with glass obtained from Switzerland. Actually the real reason was the completely prohibitive import duty placed by our government on glass. In 1845, however, what has been aptly referred to as " the astronomical window tax," was repealed. Some years after this Thomas Cooke, the self-educated son of a shoemaker in Yorkshire, became expert in lens making, and finally in 1868 completed for the late Mr. Newall's private observatory at Gateshead a telescope 25 inches in diameter. This magnificent instrument is still the second largest visual refracting telescope in England and the chief telescope at the Solar Physics Observatory at Cambridge. Its maker was the founder of the firm of Cooke, of York, recently amalgamated with Troughton and Simms.

Shortly after this Alvan Clark again took the lead and built the 26-inch telescope for the United States Naval Observatory, with which the two satellites of Mars were discovered. Then in 1880 the 27-inch for the Vienna Observatory was made by the English optician, Howard Grubb, founder of the present firm, Sir Howard Grubb and Parsons and Co., Ltd.

At last, about 1883, the imposing diameter of 30 inches was reached by Alvan Clark, in the great telescope for the Imperial Observatory at Pulkowa. In the following year, Henry and Gautier of Paris built the refractor of 31·5 inches for Nice, and in 1891 the same firm completed the 32·7-inch for the Meudon Observatory in Paris. But the largest of all—the largest refractors ever made or ever likely to be made—came again from the workshops of the Clarks in America. In 1888 they finished the 36-inch for the Lick Observatory on Mount Hamilton in California, and in 1897 the 40-inch for the Yerkes Observatory near Chicago.

This triumphal procession up the scale of apertures from 15 to 40 inches occupied the last four decades of the century. But though the increase in the *size* of refractors was an important achievement, the rapid increase in the number of large refractors was even more important. In 1861 no achromatic telescope exceeded 15 inches; yet by 1900 there were forty-one over 15 inches, twenty-three over 20 inches, and five over 30 inches!

Now there is an upper limit to the size of a telescope lens. A lens can only be supported round its margin, so after a certain size has been reached the glass will begin to become distorted by bending under its own weight. To some extent this may be avoided by increasing the *thickness* as well as the diameter of the lens. But here again there is a limit. Sooner or later the advantage gained by the increasing diameter (by which means more light is collected) will begin to be outweighed by the disadvantage of increasing thickness (which causes an increasing loss of light by absorption in the glass). Now although the 40-inch lens of the Yerkes telescope does not reach this theoretical limit it does begin to approach it. In other words whereas the difficulty, the labour and the expense involved in making an appreciably larger lens would be terrific, the advantages obtained would be negligible in comparison. In fact it is interesting to note that since 1900 no refractor exceeding 30 inches in diameter has been made. Doubtless in the future a few more refractors comparable with those at Yerkes, Lick and Meudon will be erected for special purposes, but it is extremely improbable that any attempt will ever be made to construct anything appreciably bigger.

Fortunately this did not mean that astronomers had reached the limits of telescopic power. For it was only the refractor that had reached a size beyond which it was unlikely to increase. About the time that Alvan Clark was experimenting with his first small lenses in America, there was invented independently in Germany and France a new sort of mirror for the reflecting telescope. In 1856 the German optician Steinheil, and in the following year the great French physicist Foucault, found independently that a reflecting surface of unexampled efficiency could be obtained by depositing chemically on a polished glass surface an extremely fine film of silver. The polished metal mirrors of the Herschels and Rosse returned to the eye not much more than half the amount of the light which originally fell upon them. But the new silver-on-glass mirrors of Steinheil and Foucault reflected rather more than ninety per cent. of the light incident upon them. Another great advantage of the new device was the ease with which the brilliance of the surface, so prone to become tarnished, could be restored. The surface of the mirror of a reflecting telescope has of necessity to be exposed during use to the night air; and sooner or later it is bound to become tarnished. To restore the former brilliance of a metal mirror is a long, complicated and often risky process, for it entails not only polishing but also refiguring the surface. But with a silver-on-glass mirror it is simply a question of removing the old film of silver with strong acid, chemically cleaning the surface, and finally redepositing a fresh silver film. With the metal mirror the process will involve many days or weeks of highly skilled labour; with the glass mirror the re-silvering can be done very easily and takes even with large instruments only an hour or so. Lastly, in the actual making of mirrors the original grinding, figuring and polishing are easier with glass than with metal, and the weight, size for size, is very much less.

In the days of metal mirrors the reflecting telescope had never come into general use among astronomers. The majority of astronomers at that time were concerned with measurements of the positions of the heavenly bodies; and for that they did not require the great light-gathering power of a large telescope. A small achromatic telescope was quite sufficient for the purpose, and at an earlier date sights or pointers were often preferred to the non-achromatic telescope with its single lens. The Herschels of course were exceptional. Their line of investigation was at the time quite unexplored and did not enter as a part of routine astronomy until long afterwards. Yet for its success the essential requirement was the great light-gathering power of a large aperture; and the only way in which this could be obtained was by building large metal reflectors. And these same considerations much later forced Lord Rosse and William Lassell to follow their example. It is a striking testimony to the patience and skill of these four men, that they alone among astronomers achieved with this instrument noteworthy results.

The new method of silvering glass mirrors completely changed the situation. Previously unless a reflector was of quite exceptional size its disadvantages outweighed its advantages. Henceforth reflectors of more moderate dimensions could compete on a more or less equal footing with refractors. The silver-on-glass mirror had certain definite advantages over the lens. The factors which limited the size of a lens did not apply to mirrors. In the first place a mirror can be supported at its lower surface so that it will not be distorted as a lens is by its own weight. An astronomical mirror, unlike a looking-glass, reflects the light from its *front* surface; hence there is no loss of light due to absorption by the thickness of the glass, as there is with a lens. Again, since the light is reflected from the front surface of the mirror the optical perfection of the glass need only be " skin deep ": thus although one requires good quality glass for a reflector it need not possess that rare and extreme degree of perfection required in the glass discs for a lens. Finally whereas it is necessary to grind, polish and figure four glass surfaces for an achromatic lens, one is only concerned with a single surface when grinding, polishing and figuring a mirror. For these reasons a reflector can be made much more easily and cheaply than a refractor and, further, its size can be increased if not without limit at least far beyond the limits of a refractor.

It was therefore scarcely surprising that the new type of reflector rapidly became popular. It was, particularly in England, the historic home of the reflector, that the mirror makers of the new era made the greatest strides. Among the first were With, of Hereford, and Calver, of Chelmsford, whose skill in achieving such perfect optical surfaces gained them world-wide fame. With's largest reflector, which has a diameter of 18 inches and is of excellent quality, is now at the observatory of the Rev. T. E. R. Phillips at Headley, Surrey. In 1879 Calver made for A. A. Common the 3-foot mirror with which he did his pioneering work in celestial photography. Later this telescope, the mounting of which was made by Common himself, was sold and presented by Crossley to the Lick Observatory. With it Professor Keeler, the Director of Lick, made his famous photographic survey of the nebulae; and it still ranks among the most important of photographic instruments. The 3-foot Calver-Common reflector—now generally known as the " Crossley Reflector "—was the first of the great silver-on-glass reflectors. It is interesting that after half a century it was the first large reflector to have its silver film substituted by the modern aluminium film. For in 1933 it was aluminized—a very important modern process which as we shall see has still further increased the efficiency of reflectors.

Though no large reflector during the nineteenth century achieved such perfection as the Calver-Common 3-foot a number were built of much larger size. A 4-foot reflector was made by Martin in France in 1875; and in 1891 Common himself made and mounted at his observatory at Ealing one 5-feet in diameter. But Common, apart from his photographic work, was primarily an engineer and though he made many important contributions towards the construction of the mountings of large reflectors his 5-foot mirror was not of the highest perfection. This instrument, for which Common afterwards made a second mirror, went subsequently to Harvard : but it was not until a few years ago that one of its two mirrors, with its surface refigured, began to take an important part in astronomical research. Now at the Harvard Station at Bloemfontein it is occupied in one of the most important of astronomical investigations, the photographic survey of the faint nebulae of the southern hemisphere. The other mirror has also been refigured and remounted at the new Harvard Station at Oak Ridge, Massachusetts.

But though the reflector in its new garb of silver-on-glass was beginning to make headway in the race with the refractor for ever larger apertures, its most noteworthy contribution to astronomy at this period was of a different kind. It acted as a tremendously powerful stimulus to amateur astronomy. In the past the amateur, as we have seen, had played an important part; but the number of amateurs was small. Now, however, this number was to become legion. The cost of a reflector of moderate size is perhaps one-fifth of that of a refractor of the same diameter. The man of humble means who had previously been limited to a refractor from three to five inches in diameter could for the same price obtain a reflector 12 inches or more in diameter. Moreover with reasonable patience and skill he could make and mount such an instrument with his own hands. We thus find the end of last century marked by a rapidly spreading interest in astronomy and by the appearance of a great number of amateur astronomers with instruments, often home-made, which were capable of doing valuable astronomical work. An event which bore witness to this new movement was the foundation in 1890 of the British Astronomical Association. Its membership was, and still is, largely recruited from among the amateurs, and its publications are to a great extent a record of their observations. A small refractor is very limited in the amount of useful work it can do; but a reflector with a diameter in the neighbourhood of 12 inches may in skilful hands accomplish a great deal of useful work. Thus with instruments of this type much has been added to our store of knowledge; and this is specially the case in the study of variable stars, in the study of the planetary surfaces and in the discovery and observation of comets.

CHAPTER V

NOVAE AND THE MEASUREMENT OF STELLAR BRIGHTNESS

In this chapter and those which follow it we shall have to tell as in the previous chapters of new inventions and new discoveries; but our main task will be in tracing out the gradual development of a new outlook: the tackling of astronomical problems generally from a broader and more comprehensive point of view, and the resulting emergence of a much fuller and more unified understanding of the universe.

In the previous chapters we traced the steps that were to make this possible. We saw first, how, about a hundred years ago, astronomy had set about putting its house in order. Its recent acquisition of more precise means of observation had necessitated its overhauling and strengthening those ancient foundations laid down during the previous two thousand years. So it determined more accurately the positions of its reference points, the fixed stars, and increased greatly the number of such measurements. At the same time it attacked more effectively numerous problems—some old, some new—of individual interest. It improved the observation and theory of the motions of the moon and planets; it discovered new members of the solar system; it studied the surfaces of the planets, of the sun and of the moon; it investigated and explained the mutual revolutions of double-stars; and it discovered among the fixed stars some that were near enough to measure their distances and their minute movements in the heavens. And then on top of all this the spectroscope and the photographic plate came into existence and enormously enhanced the power and scope of astronomical investigation. The spectroscope gave to the astronomer knowledge of a novel kind, and opened to him the wide gates of celestial chemistry. It now became possible for astronomers to investigate the composition of the sun and stars, and to make their first guess at their past and future history. They became possessed of the means for measuring the velocities of the celestial bodies towards and away from the earth, and for investigating the mutual revolutions of twin suns, the duplicity of which no telescope was powerful enough to reveal. Photography brought to them an extended range of vision, increased accuracy of measurement, and above all economy in their expenditure of time and labour. With these attributes, and the additional one of rendering observations by different observers, in different places, at different times more homogeneous, photography enabled astronomers to organize on an extensive scale the mass production of observations.

So grand a scheme naturally entailed, as we saw, much heavy spade-work and the exercise of considerable patience before its results could materialize. But now, at last, the labour begins to be rewarded, observations are flocking in in prodigious quantity, and the application to them of statistical methods is surpassing the most sanguine expectations. It is with these matters that the subsequent chapters will be mainly concerned.

In the first year of the new century, in the early morning of February 22, 1901, Dr. Anderson, an amateur astronomer in Edinburgh, noticed in the constellation of Perseus a moderately bright star which had not previously been there. By the following night its brightness had increased so that it was almost the brightest star in the sky. But the next night it had started to fade slowly, and a year later it was barely visible to the naked eye.

This remarkable apparition, the sudden temporary appearance of a " New Star " or nova as it is called, was not unique. There had been several recorded in the past, but for nearly three hundred years none had been seen which had anything like the brilliance of Nova Persei. Before discussing the more detailed history of this remarkable object we must review briefly the more important results which had been obtained from the study of previous objects of the same sort.

A nova is not in any real sense a *new* star; it is rather a star which has suddenly and very quickly become enormously brighter. At its brightest a " new star " may reach almost any brilliance, depending roughly upon how bright it was before its outburst. But even the novae that have reached the greatest brightness were all far below naked eye visibility before they started to flare up. Consequently in pre-telescopic days there could be no evidence to show that a nova had previously existed as a faint star; and so it was that they naturally came to be called novae or " New stars."

Unfortunately in the earliest records it is not always stated whether the new object that appeared in the heavens was stationary or was moving among the stars. If it was fixed in the sky it would undoubtedly have been a " new star," but if it was in motion it would have been a comet. Doubtful records of this kind are to be found in some of the early Chinese annals. Again there was the " new star of Hipparchus " in 134 B.C., which some authorities consider was almost certainly a comet. In later times the descriptions become more definite; and between A.D. 100 and 1300 the late Miss Agnes Clerke lists nine novae which she regarded as fairly certain. All of these must have been objects of great brilliance, and one of them, in A.D. 389, is said to have rivalled the planet Venus. But the first recorded nova about which there can be no doubt was the "star of Tycho" in 1572. No nova since then has compared with it in magnitude. It certainly shone for several days the equal or even the superior of Venus. The next observed was Kepler's nova in 1604, which appeared for a time as bright as Jupiter. After that there was a gap of nearly three centuries before another nova of the first rank appeared; and that was the star we have just referred to, Nova Persei, 1901.

But in the meantime as a result of the closer watch that was being kept upon the heavens a number of somewhat fainter novae began to be recorded. In 1670 a Carthusian monk named Anthelmus was so well acquainted with his stars that he succeeded in detecting a new object among the numerous and comparatively faint third magnitude stars. But that was a remarkable achievement not likely to be repeated for a long time; and in fact it was not until 1848 that another comparatively faint nova was discovered. This object which was discovered by Hind, who was the observer in charge of a private observatory in Regent's Park, reached at its brightest only the fifth magnitude. During the remainder of the nineteenth century three more of these "second class" novae were found rising respectively to the second, third and fourth magnitudes. One thus sees that while the outburst of a really great nova is an excessively rare occurrence the appearance of a faint nova is a much more frequent event. And this supposition as we shall see presently is amply borne out when we take into consideration the still fainter novae that have been revealed by photography.

Before 1848 nothing was known about novae except that they blazed up with extreme suddenness and then faded more slowly away. Hind discovered his "new star" during his search for new minor planets of which he had already found two. This work entailed the careful comparison of the positions of faint stars with their positions on existing charts; and it was in this way that he was able to detect the presence of a new fifth magnitude star on April 27, 1848, and to state definitely that on April 5 no star as bright as the ninth magnitude was in its place. A day or so after the outburst, which these observations show must at least have been fairly rapid, the star began to decline slowly after the manner of its more brilliant predecessors. In the months during which it was fading the star assumed a most intense red colour; but though this red colour had been referred to in some of the historical records, it was not until much later that it was realized that all novae become red in their earlier declining phases. Another point which issued from the apparition of 1848 was the remarkable way in which novae invariably appeared in the immediate vicinity of the Milky Way. Attention to this important fact was drawn by Hind himself who showed that all the recorded novae, with only a few exceptions which were probably comets, obeyed this rule.

The next " new star " was Nova Coronae Borealis which was discovered in 1866 by Birmingham in Ireland. It was a discovery of great importance for this was the first object of its kind to have appeared since the invention of the spectroscope. Within four days of the discovery, Huggins was able to carry out the unique experiment of subjecting its light to analysis. The result was remarkable. Superposed upon a spectrum consisting of numerous dark absorption lines on a bright background, were a number of intensely brilliant coloured rays. Some of these bright lines Huggins identified with those of hydrogen. There could be little doubt that the outburst was due to a vast eruption of glowing gases, but as to what was the immediate cause of the eruption nothing could be inferred. Although the star was as bright as the second magnitude at discovery it remained visible to the naked eye for little more than a week.

The unusually rapid fading of this star was particularly unfortunate, for it naturally cut short the spectroscopic studies which at that period were only applicable to bright objects. After an interval of ten years another " new star " was discovered, Nova Cygni 1876. Once more the spectroscope revealed the curious spectrum of dark lines with intensely bright lines superposed. But it was about ten months after the outburst when the star had sunk to the tenth magnitude that the spectroscope made its most startling revelation. In place of the previous double spectrum with the bright and dark lines of hydrogen and other elements, there now stood a solitary bright green line, that line of unknown origin which was typical of the gaseous nebulae. Thus a star had been seen to turn into a nebula, and the orthodox theory of stellar evolution appeared to have been turned completely upside-down.

So far however the nova spectra had only been observed visually; no permanent spectroscopic records had been obtained, nor could any accurate measurements of the positions of the spectral lines be made. It was only in 1891 when a "new star" appeared in Auriga that the photographic and spectroscopic methods were united for the first time in an attack upon the nova problem. And the advantages of the double method already recognized in other departments of the science were at once found to be of especial value in this one. The photographic plate was able to bring out details and delicate contrasts in the spectrum which were unavailable to the eye; on it the positions of the lines could be measured with far greater ease and accuracy than they could be at the eye-piece; and above all it secured and made permanent with a short exposure what ordinary mapping could only have accomplished after many nights of work.

Nova Aurigae was discovered by Dr. Anderson, to whom we have already referred as the discoverer ten years later of Nova Persei. Since the star was then only slightly brighter than the fifth magnitude the achievement was a most noteworthy one. It is true that Hind's nova was even fainter, but the discovery on that occasion was in a sense more or less accidental. Hind's nova happened to appear in a region which he was charting for another purpose. But Dr. Anderson was specifically looking for new objects. And when one remembers that there must be about a thousand stars brighter than the fifth magnitude visible in the skies of Edinburgh, one realizes that the detection of a single new one among them must imply an almost uncanny acquaintance with their configurations.

Nova Aurigae was already on the decline when the first photographs of its spectrum were secured. The striking feature was that the large number of bright coloured lines, including all those of hydrogen, were accompanied in the majority of cases by dark lines, like shadows of themselves, upon their violet sides. Almost to its finer details the spectrum was double, and appeared to consist of a positive and a negative copy of a single spectrum superposed, but not exactly, upon each other. When the plates were measured it was found that the displacements of the lines indicated that the absorbing gases were approaching the earth at more than 200 miles a second while the hotter gases emitting the bright lines were receding from us at over 300 miles a second. This naturally suggested that two stars were involved, one with a bright line and one with a dark line spectrum, and that they had happened almost to collide; at the time of closest approach they must have nearly grazed each other's surfaces, and as a result of the friction, or of the mutual tidal forces engendered, a gigantic luminous eruption had occurred. It was soon seen, however, that such a theory was difficult to accept. One had to suppose that two bodies, which must originally have been unconnected with one another and separated by vast distances, had just happened to be travelling exactly towards one another. There was also the extraordinary coincidence that in these completely independent stars the spectrum of one was almost a precise replica in negative of that of the other. But when the spectrum came more carefully to be examined the lines were found to be not merely double but triple and later even quadruple. Finally, as the late Miss Agnes Clerke wrote, " Victor Schumann pointed out that not less than six, if as many as two, stars were engaged in the outburst; and planets *ad libitum* were thrown into the *mêlée* by Vogel." Thus was this simple theory of Nova Aurigae reduced to an absurdity.

But though no satisfactory theory could be found to fit in with the detailed spectroscopic phenomena of the new star a number of quite plausible suggestions were put forward as to how in a general way the conflagration had been brought about. One view was that the outburst originated spontaneously within the star and was similar to such disturbances as produce sun-spots though on a much bigger scale. Another view favoured some form of collision; and since it was extremely unlikely that two stars should happen to collide it was supposed that the sudden brightening of a star might be caused by its passage through one of those diffuse nebulous clouds which abound in the direction of the Milky Way and fill such enormous volumes of space. In favour of the latter hypothesis was the fact already mentioned that new stars invariably appeared in or close to the Milky Way. On the other hand both hypotheses quite failed to explain why the outbursts of novae were so similar to one another and why the outburst of a typical nova was an isolated event which was never repeated.

In its subsequent behaviour Nova Aurigae differed from its predecessors. Instead of entering immediately upon a rapid decline, it retained for many weeks the greater part of its light at maximum brightness and only then started to fade quickly. Within two months of the commencement of this precipitous fall it had reached the extreme limit of visibility with the Lick 36-inch refractor; and since by that time the glare of the spring sunset was already encroaching upon that particular part of the sky, it was thought that the last view of it had been obtained. But not a bit of it: in the dawn of the following autumn it was found to have shot up again to the tenth magnitude while its spectrum had been transformed like the spectrum of Nova Cygni into that of a nebula. The importance of this observation lay not in its novelty but in the definiteness of its establishment. The metamorphosis of the earlier nova had been observed visually, that of Nova Aurigae had been recorded photographically; and the photographic plates which brought out numerous spectral lines which the eye could not detect proved beyond doubt that the likeness to the nebular spectrum was not merely a superficial one.

Yet the enthusiasm displayed during the apparition of Nova Aurigae was mild in comparison with that aroused by the outburst to which we have already referred of Nova Persei in 1901. It has been said that whole libraries could be filled with the spectral plates which were secured of it. The outburst in Auriga had whetted the appetites of the astronomers for a fuller knowledge of the curious nova-phenomenon; and so soon as it was announced that Nova Persei had appeared astronomers fairly leapt on it. Of course in the ten years that had elapsed there had been numerous improvements in the spectroscope and in photographic plates; so the instruments which were available in 1901 were much more numerous and efficient. Then the star itself was far brighter than Nova Aurigac and could on that account be studied more easily and in greater detail. But perhaps the chief reason why Nova Persei taught us so much was the fact that it was the first nova to be caught spectroscopically while it was still on the rise to its maximum brilliance.

At discovery Nova Persei was slightly brighter than the third magnitude. A photograph which happened to have been taken twenty-eight hours earlier of the same part of the sky showed that it was then certainly not brighter than magnitude 12. Thus in that short period the star had brightened up by at least nine magnitudes or about four thousand times. A day and a half after discovery it reached its culmination as one of the brightest stars in the sky and attained magnitude 0. Immediately after this it started on a fairly rapid decline broken by minor fluctuations.

During the first two nights after discovery, while the light of the star was still increasing the spectrum was found to be that of a normal star of the hydrogen type. Superposed upon a continuous band of colour were the typical dark absorption lines of hydrogen. The beautiful duplicated bright and dark line spectrum, previously regarded as typical of novae, was strikingly absent. Yet within another twenty-four hours a complete transformation had occurred: the light of the star was now beginning to decline, and in its spectrum shone the bright radiations of hydrogen, each one bordered on its violet side by a dark absorption line. And so for the first time it appeared that the characteristic nova spectrum was but a phase in a more general phenomenon and only came into being when the ebb of the light-tide began to set in. Measurements of the photographs showed that although the bright hydrogen lines were enormously widened, the centres of them were close to their normal positions, thus indicating that the gases producing them were *on the average* stationary, neither approaching nor receding from the earth. The displacement of the corresponding dark lines was very large and increased considerably for several days; it finally gave for the absorbing gases a velocity of approach of as much as 1,000 miles a second. About three weeks after discovery these dark lines on the violet sides of the bright lines were found to have become split up into numerous fine lines. Thus the multiplicity of the spectrum first seen in Nova Aurigae was repeated in Nova Persei and suggested that each of the absorbing gases was present in several layers approaching us with very different velocities.[1] Presently a new phenomenon was discerned: it was seen that the minor ups and downs in the light of the star which accompanied its fading were closely paralleled by changes in the spectrum. Whenever the star brightened the spectrum tended to revert to the pre-maximum type of spectrum —the continuous background brightened and the dark lines grew more intense; and whenever the star faded again the continuous background grew paler and the bright lines more conspicuous. As the oscillations died out and the star became fainter the spectrum came to consist almost wholly of bright lines. Then finally the bright lines were gradually transformed, the lines of hydrogen and of the metals fading away and those of the nebular spectrum taking their place. By July, six months after the outburst, the usual nebular metamorphosis was complete.

The story of Nova Persei was not, however, by any means ended. Still stranger events were to follow; and curiously enough these were revealed not through the intermediary of the spectroscope but by simple direct photography. Towards the end of August Wolf took long exposure photographs of the nova with the rapid star-cameras at Heidelberg, and found a nebulous arc partly surrounding the star at a considerable distance from it. A few weeks later other photographs taken in America showed the arc to be expanding. From a knowledge of the distance of the nova, which had recently been measured, and from a comparison of the two sets of photographs, the speed at which the arc was creeping outwards was quite easily calculated. The result at first seemed to be incredible, for the velocity came out almost as great as the velocity of light itself. Such an achievement even on the part of a celestial explosion was quite out of the question; besides, the spectroscope had already measured the velocity of the ejected gases and got the large but reasonable figure of about a thousand miles per second. No, the apparently expanding nebula was really stationary and had been there, though invisible, all the time. And the movement that was seen was the movement of the light-waves which, spreading outwards from the star, like the widening ripples on a pond, successively illuminated the surrounding nebulous clouds. It was as though we were watching a distant wave travelling over the surface of the sea, which, wherever and whenever it reached a rock, revealed its presence by the white spray thrown up in its breaking. This phenomenon—unique in astronomical observation—brings home to one more vividly than any other the immensity of interstellar distances. For the light-waves which crept outwards so slowly that a week or two had to elapse before the motion could be detected, were really travelling with the velocity of light which would carry them eight times round the world—or 186,000 miles—each second.

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[1] That the multiple nova spectrum was due to the successive ejections of gas from a single star was first suggested by Bickerton after the outburst of Nova Aurigae in 1892.

It was nearly sixteen years later that the curtain was rung up on the final act of this celestial tragedy. In September 1916, while examining the star with his 15-inch reflector at West Norwood, Dr. W. H. Steavenson noticed that its star-like image was embedded in an ill-defined nebulous disc. In December of the same year this change was independently noticed by Barnard at the Yerkes Observatory, who with the aid of the great 40-inch refractor was able to make out more clearly the detailed shape of the minute nebulosity and to determine its precise extension. By 1920 this same observer was able to show beyond doubt that the tiny nebulous disc was gradually growing larger, and that the rate at which it was increasing corresponded with the velocity which sixteen years earlier the spectroscope had given for the approach of the absorbing gases in the star's atmosphere. Thus the transformation of a star into a nebula, first suggested by the spectroscope, had received very striking visual confirmation.

Magnitude Measurements. We must now consider the advances made in the measurement of stellar magnitudes since the days of Argelander. Up to about 1872 the magnitudes of stars were only to be determined by simple eye-estimates. Sequences were formed in various parts of the sky in which the stars were arranged in order of their relative brightness; these different sequences were brought into relation by means of cross-sequences; and finally a magnitude scale was fitted to the combined sequence so as to deviate as little as possible from the classical Ptolemaic magnitudes. But in such a vast undertaking as the *Bonner Durchmusterung* it was obviously impossible to determine for the three hundred thousand odd stars individual magnitudes by this laborious method. Instead, Argelander and his assistants had had to rely on what one may best describe as a sense of " absolute brightness " analogous to the sense of " absolute pitch " developed by musicians, and the sense of " absolute velocity " recently acquired by police officers. From their immense experience they were able to say, when a star sailed into the field of view, what its magnitude was to about one-fifth of a magnitude. Generally they were correct; but naturally they sometimes made a mistake.

But about 1870 astronomers in England, Germany and America began to consider the possibility of applying the principles of precise *photometry* to the measurement of the brightness of the heavenly bodies. The underlying idea of photometry in comparing the brightness of two objects is to make use of some optical device for reducing the brightness of the brighter object *by a known amount*—until as judged by the eye the two are equally bright. One of these devices consists in the introduction of a wedge-shaped piece of tinted glass between the eye and the brighter of the two objects. The thicker the portion of the wedge that is interposed, the greater is the reduction in the brightness of the object. One simply adjusts the position of the wedge until the two objects appear equally bright; and then, by noting the thickness of the portion of the wedge that has been interposed, one deduces at once how much brighter the brighter object is than the fainter one. The other device depends on the phenomenon of " polarization." When a ray of light is passed through a crystal of "Iceland Spar" its properties are subtly altered: to the eye the ray is unchanged, but if it is now made to pass through a second crystal of Iceland Spar it will in general be reduced in brightness. The amount by which the brightness is reduced depends on the relative position of the " axes " of the two crystals: if the axis of the second crystal is parallel to that of the first the reduction in brightness is zero; if the second crystal is rotated till its axis is at right angles to that of the first the reduction will be complete and no light will get through. At intermediate positions the reduction is partial, and if we measure the angle between the two axes we can calculate the amount by which the light has been reduced. Thus by a suitable polarizing device we can cut down the light of the brighter object until it is equal to that of the fainter one and work out as before the difference in brightness between the two.

Between 1872 and 1877 preliminary experiments were made in the application of such methods to astronomy in England and America, but the man who first reduced them to a practicable basis was E. C. Pickering who had recently been appointed director of the Harvard College Observatory. Between 1877 and 1879 he mounted a photometer on the 15-inch refractor at Harvard and used it for measuring the relative brightness of the components of double stars and of the satellites of the planets. So far, however, the method was confined to the comparison of two objects which were close enough to each other to be visible together in the same telescopic field. But in 1879 he devised another instrument for comparing with one another two stars which were situated in *different parts* of the sky. This was called the " Meridian Photometer."

With this instrument the brightness of any star in the sky could be compared directly with that of the Pole Star by bringing the images of the two stars simultaneously into the same field of view. A horizontal tube was fixed rigidly east and west. At one end of the tube two telescope object-glasses were placed side by side, while set into the other end of the tube was the *single* eye-piece. By means of an arrangement of mirrors or prisms inside the tube the light coming from each of the two object-glasses was reflected simultaneously into the common eye-piece. Immediately in front of each of the two object-glasses a mirror was mounted at an angle of 45 degrees to the tube, and arranged in such a way that it could be rotated about the axis of the tube without changing its 45-degree tilt. On looking into the eye-piece of the double telescope one would see simultaneously in the field of view two separate portions of the sky; and by suitably rotating the two mirrors one could select the two portions from anywhere along the line running directly overhead from the north horizon to the south horizon.

Since the Pole Star has a practically fixed position on this north-south line it could always be brought into the field of view by one of the mirrors. Then with the second mirror one could bring into the same field any other star that happened to be crossing the north-south line, or " meridian," at the time. Thus all the stars that were visible from a given locality came within reach of this instrument; for it must be realized that when a star is crossing the meridian of a place it is always at its highest above the horizon of that place.

Having brought the Pole Star and the other star into the field by a suitable rotation of the mirrors, the observer reduces, by means of a polarizing device in the eye-piece, the brighter of the two images—usually that of the Pole Star—until they appear equal. From the angle through which he has to rotate the crystal to give this equality he calculates the difference in brightness between the two stars. But first of all a correction must be applied. The atmosphere has a dimming effect on the brightness of a star which is more marked the lower the star is in the sky. Since the Pole Star and the star being compared with it are not usually at the same height above the horizon, one allows for the difference by applying to the result a correction which has to be determined by experiment.

Between 1879 and 1881 Pickering with the Meridian Photometer compared the brightness of all the naked eye stars visible at Harvard with that of the Pole Star. He accepted of course the orthodox definition that if two stars differed by one magnitude their brightnesses were in the ratio of 1 to 2·512—*Pogson's Ratio*. But though the *scale* of his magnitudes was thereby determined, there was as yet no precise agreement as to their " zero-point." The fixing of the zero-point is necessarily arbitrary : but one must be precise in stating where it is to be arbitrarily fixed. It is all a matter of convenience. For a quarter of a century the magnitudes of the *Bonner Durchmusterung* had reigned supreme ; and even now that Pickering had obtained considerably more reliable magnitudes for some 4,000 of the brighter stars, they would still reign supreme for the remaining 320,000 fainter stars. It was essential therefore that the fixing of the zero-point should upset as little as possible this great system of the Bonn magnitudes. Pickering achieved this by fixing his scale so that his own and the Bonn magnitudes were in agreement in the range in which the stars he had observed were most numerous. Since the number of stars increases rapidly with diminishing brightness, and since the faintest stars which Pickering had measured were around the 6th magnitude, the scale was adjusted so that at the 6th magnitude the Pickering and Bonn magnitudes would be the same. It speaks well for the work of Argelander and his assistants that for the Pole Star—which is near the upper end of the scale where considerable divergence of the two systems might have been anticipated—Pickering got a magnitude of 2·1,while the Bonn value was 2·0, only one-tenth of a magnitude different.

These results of Pickering were published in 1884 as the *Harvard Photometry*, a catalogue giving the magnitudes of 4,260 stars from the north pole to thirty degrees south of the equator, complete to magnitude 6·0 or 6·5. Thus Pickering laid the foundations of precise stellar photometry : he gave us the first definite magnitude system, defining its scale by Pogson's ratio, and fixing its zero-point by defining the magnitude of the Pole Star as 2·1 ; and he gave us the first catalogue of standard photometric magnitudes to form the basis of all subsequent catalogues.

A similar investigation was started in 1881 by Professor Pritchard at the Oxford University Observatory. In it he used a " wedge photometer," and compared the brightness of the stars by noting the thickness of the tinted wedge which was just sufficient to extinguish the light of the stars. The *Uranometria Nova Oxoniensis* which gave the magnitudes of 2,784 naked eye stars from the pole to ten degrees south of the equator was published in 1885. And in the following year Pickering and Pritchard were jointly honoured by the award of the Gold Medal of the Royal Astronomical Society.

Both the Harvard and Oxford catalogues had been in the nature of preliminary investigations : rather than aim at the highest attainable precision, they had attempted to supply quickly, with reasonable accuracy, an urgently felt need. But another investigation of a similar though more precise kind was already getting under way at Potsdam in the hands of Muller and Kempf. Like Pickering they used a polarizing photometer, but instead of comparing each star directly with the Pole Star, they compared it with an " artificial star " illuminated by a constant source of light. The first quarter of this catalogue was published in 1894, but the final volume only came out in 1907. The *Potsdamer Photometrische Durchmusterung* gives the magnitudes of 14,199 stars between the pole and the equator down to magnitude 7·5 ; and the individual magnitudes can probably be relied on to less than 0·1 magnitude.

The photometric work of the Harvard Observatory did not by any means come to an end with the publication of the *Harvard Photometry*. So soon as the observations for it had been completed, work was begun with a second more powerful meridian photometer on the measurement of the fainter stars in the *Bonner Durchmusterung*. Since it would have been impracticable to measure in this way all the stars in the Bonn catalogue, sample regions were chosen from every part of the sky and in each of these all the Bonn magnitudes were re-measured down to the faintest limits. It was known that Argelander and his assistants had departed considerably from Pogson's ratio in the estimation of their fainter magnitudes. But by measuring accurately the stars in these sample regions it was possible to obtain a set of corrections by which the Bonn magnitudes for the fainter stars could be brought into line with the *Harvard Photometry.* This task which entailed the measurement of over 20,000 stars took from 1882–1888.

It was at this point that the enormous enterprise which was so characteristic of Pickering came most clearly into view. Almost immediately the meridian photometer had accomplished its allotted task at Harvard he had it packed up and sent, in the charge of Professor S. I. Bailey, to Peru to extend the *Harvard Photometry* to the southern hemisphere. Bailey spent three years in South America, from 1889 to 1891, and completed in that short time a tremendous piece of work. Four times he set up his improvised observatory and four times he transported his instruments over great distances, often on the backs of mules through country where there were no roads. Finally he settled at Arequipa, which afterwards became the official southern station of the Harvard Observatory. During the whole period he was far removed from all scientific assistance and had to rely on his own resources to overcome the numerous difficulties which inevitably arose. Yet he completed the *Harvard Photometry* so that it covered the entire sky, by measuring the magnitudes of the 1,428 naked eye stars from thirty degrees south of the equator—the southern limit of the northern photometry—right down to the south pole. And in addition he measured 6,494 fainter stars to about the 10th magnitude in a specially selected belt of the sky. This done he returned to Harvard, taking the photometer with him.

But still this was not all. Directly the photometer arrived home Pickering started to observe the *Harvard Photometry* all over again, and to extend its previous naked eye limits to include all stars down to magnitude 7·5. Nearly half a million observations were made, mostly by Pickering himself, and the programme was completed in 1899. But it was necessary to carry out this revision and extension in the southern hemisphere as well; so back to Peru in 1899 went Bailey and the photometer. The final upshot of this gigantic campaign which had taken twenty-three years (1879-1902) was the publication in 1908 of the *Revised Harvard Photometry,* which is to this day the standard photometric catalogue. It gives to one-hundredth of a magnitude the brightness of all the stars in both hemispheres, complete to magnitude 7·0 or 7·5, numbering in all 45,792.

It will probably have struck the reader as odd that there should be all this ado about the magnitudes or apparent brightness of the stars. Why should there be all this time, labour and money spent on measuring what is obviously only a " secondary quality " and in no sense a fundamental attribute of a celestial body? Of course the apparent brightness must be important for the study of variable stars. We shall see, however, that it is of even greater importance for quite other reasons. As we push further into the more remote parts of the universe, our knowledge of the heavenly bodies—of their distances, their sizes and even their weights—becomes ever more intimately bound up with precise measurement of their apparent brightness. But if this is so it would seem that we ought to possess accurate magnitudes for all stars in the sky down to the extreme limits visible with our largest telescopes—about magnitude 20. But such an undertaking is quite out of the question. For each step of one magnitude which we take down the scale the number of the stars is increased about threefold; and when we get to magnitude 20 it is estimated that the number of the stars totals about a thousand million. As far as magnitude 7 or 7·5 our data are complete: for the northern hemisphere they are given by both the Harvard and Potsdam photometries, and for the southern hemisphere they are given by the Harvard Photometry. But it is scarcely practicable to suppose that we shall ever get complete data to a limit much lower than this. Exactly the same problem arises in regard to all other kinds of astronomical data. It is quite impracticable to obtain measures of positions, measures of distances, determinations of velocities and studies of spectra of a thousand million stars. And yet we cannot stop short at some arbitrary limit of magnitude and say that beyond this we shall not go. For if we did so our statistical studies, on which so much of our knowledge of the universe depends, would soon lead us seriously astray. It was to meet this difficulty that Kapteyn in 1906 proposed his now famous " Plan of Selected Areas." He selected 206 small areas regularly distributed over the sky, and forty-six additional areas of special interest. The scheme in which a large number of observatories are co-operating is to make studies of every description—of brightness, of position, of distances, of velocities, of spectra, etc.—of all the stars in each area *from the brightest to the faintest.* When this is completed—though with increasing instrumental power results can always be improved and the scheme extended to ever fainter stars—the power of the statistical method should be enormously enhanced.

CHAPTER VI

THE MEASUREMENT OF STELLAR BRIGHTNESS AND ECLIPSING VARIABLE STARS

IT thus came about that even when the *Revised Harvard Photometry* had been finished there still remained work to be done with the meridian photometer. The original instrument had object-glasses 2 inches in diameter, the second instrument was equipped with 4-inch object-glasses; and it was with this latter instrument that most of the observations contained in the Revised Photometry were made. Soon after this work was complete Pickering designed a 12-inch meridian photometer which could measure stars down to about the 13th magnitude. And it was with this instrument that he determined the standard magnitudes of over 16,000 stars in many of the Kapteyn areas. Below the 13th magnitude there was no point in extending the work of the meridian photometer. Stars of the 13th magnitude or brighter are so numerous that one is bound to be able to find one close enough in the sky for direct comparison with any fainter star. One simply has to use a photometer attached to the eye-piece of an ordinary telescope and reduce the brightness of the image of the standard 13th magnitude star until it appears equal to the other.

So far the methods we have described for determining standard magnitudes or for comparing the relative magnitudes of two stars have been essentially *visual*. Various devices were used for sharpening the sensitiveness of the eye; but that was all: the eye always gave the final verdict. But there is another quite different method, in which the eye enters, if at all, only indirectly. This is the method of photography.

G. P. Bond, the first director of the Harvard Observatory and the immediate predecessor of Pickering, was one of the earliest pioneers in celestial photography. On any photographic negative of a group of stars one sees at once that the images of the brighter stars are larger than those of the fainter ones. In 1858 Bond suggested that it should be possible to determine the magnitudes of the stars by comparing the sizes of their images, and thereby eliminate, what was always objectionable in visual estimates, the personality of the observer. The first attempt to do this on a large scale was made by Gill and Kapteyn in the *Cape Photographic Durchmusterung*, which as we saw was started in 1885 and published in 1900. The magnitudes of its 454,875 stars were found by measuring the diameters of their images on the various plates. The diameters were translated into magnitudes by selecting on each plate a considerable number of " standard stars " the magnitudes of which had already been measured by visual methods—the sources of these visual magnitudes being Schönfeld's extension of the *Bonner Durchmusterung* into the southern hemisphere, and the catalogue of southern star magnitudes made by Gould when he went to the Argentine in 1870. Strictly speaking, therefore, the Cape magnitudes were not pure photographic magnitudes, but a sort of cross between photographic and visual magnitudes.

Now the brightness of a star not only affects the diameter of the photographic image but also the density or " blackness " of the image: the brighter the star the denser and blacker is its image. One should thus be able to compare the brightness of the stars on a negative not only by comparing the diameters of the images but also by comparing with some photometric device the density of the images, or what comes to the same thing their transparency to light coming through the plate. In addition there will be a third method which is a combination of the other two: on looking at the plate the eye receives an impression of the " general appearance " of the image, which is a combined impression of size and density. Two stars will have the same photographic magnitude if their images appear identical in this sense; and if there happens to be on the plate a number of stars of which the magnitudes are already known, the magnitudes of the other ones can be got by the simple process of " matching."

The estimation of photographic magnitudes by these three methods has been the subject of a great deal of research. The method of measuring the diameters of the images was first investigated by Charlier in Sweden and by Christie, who was then Astronomer Royal, about 1892, and also as we have just seen by Kapteyn in his work on the *Cape Durchmusterung*. About the same time the method of matching star images in regard to their general appearance was developed at Harvard mainly by Pickering and Mrs. Fleming. But undoubtedly the most fundamental work on precise photographic photometry was that of Schwarzschild in Vienna who originated the method depending on the measurement of the densities of star images. A star image on a properly focused negative is much denser at its centre than at its edge, so that it is practically impossible to assign to it any definite degree of density. Schwarzschild hit upon the ingenious plan of taking his photographs slightly *out-of-focus*. The star images then appeared as round discs of perfectly even density from centre to edge; and it became a simple matter to compare the density of any two of them.

The relations between the magnitudes of the stars on the one hand and the diameters and the densities of their images on the other were found to be very complicated. They depended on several different factors; and thus varied considerably from one negative to another. For instance, the relative diameters (or densities) of the images on one negative might differ considerably from those of the corresponding images on a second negative of the same field of stars; yet of course, apart from an occasional variable star, the relative magnitudes must be the same in the two cases. Consequently the relation between magnitude and diameter or density must be determined anew for each individual negative; and this can easily be done if there happens to be upon the negative a number of stars of which the magnitudes are already known.

In the early days, as in the work on the *Cape Durchmusterung*, the method was to select on each plate a series of " standard stars " of which the magnitudes had already been determined by some visual photometric means, and to use these to obtain for that plate the relation between magnitude and diameter or density. But, as we shall see presently, visual and photographic magnitudes do not necessarily run parallel to one another; consequently the use of visual standards in photographic determinations must give magnitudes which strictly are neither one thing nor the other. One method would be to make use, as in visual photometry, of some device by which the light of a star could be cut down by any desired and measurable amount before reaching the plate. One could then imprint upon the same plate first the star field of which the magnitudes were required, and secondly a series of star images representing known differences in magnitude from which the relation between magnitude and diameter (or density) could be derived.

This problem of determining *pure*, as opposed to what may be described as *hybrid*, photographic magnitudes was first thoroughly worked out by Schwarzschild. Now there is one way in which photography readily lends itself to the making of a scale of known magnitude steps; and that is by the simple process of varying the exposure time. One might have thought that giving a certain exposure on a faint object would be equivalent to giving half that exposure on a second object twice as bright as the first. This however is not so, and the explanation why it was not so was first given by Sir William Abney in 1893. Schwarzschild therefore set about to try to discover what the relation was between the exposure time and the brightness of a star. He found that if one star was exactly one *photographic* magnitude fainter than a second star, the density of the image of the fainter star would be equal to that of the brighter star if it were given an exposure, not 2·512 times as long (Pogson's ratio), but about *three* times as long. The factor was not exactly three, and it varied slightly from one kind of plate to another; but it was found to be remarkably constant for all plates coming out of the same " batch " from the makers.

The greater part of this work was carried out by Schwarzschild while still in his twenties, between 1896 and 1899, when he was assistant at the von Kuffner Observatory in Vienna. But it was during the next ten years while director of the Observatory of Göttingen that he applied the results of these important studies to the actual measurement of photographic magnitudes. In carrying out the *Göttingen Aktinometrie* he measured the photographic magnitudes of 3,500 stars down to magnitude 7·5 lying within a wide belt encircling the sky immediately north of the celestial equator. His method was to expose each plate three times on the same star field, shifting the plate very slightly between successive exposures. The second exposure was three times the length of the first, and the third three times the length of the second. Thus for each star there were three images close together side by side, the three images representing three steps each of one magnitude. The relative photographic magnitudes of all the stars on any one of these plates could now easily be found. We have said that Schwarzschild took his photographs slightly out-of-focus so that the densities of the star images could be compared. On a given negative all the stars of a certain photographic magnitude would have their corresponding images of equal density. All the stars exactly one magnitude fainter would have all their " number two images " equal to the " number one images," of the first group. All the stars exactly two magnitudes fainter than the first group would have their " number three images " equal to the " number one images " of the first group, and to the " number two images " of the second group, and so on. The magnitudes of stars which fell between the above-mentioned groups could be found by suitable interpolation of the density measurements. And so the relative photographic magnitudes of all the stars from the brightest to the faintest could readily be found on each individual plate. Finally in order that the magnitudes of all the negatives should be mutually comparable Schwartzschild arranged that each plate should overlap considerably the star fields covered by the plates adjacent to it so that the magnitude standard was carried on from one to the other.

There are other methods of getting a scale of known magnitude steps besides that of varying the length of the exposure. One is to use a diaphragm with an adjustable aperture in front of the object glass of the telescope. A series of exposures is given in which the length of exposure is kept constant while the size of the aperture is varied. Since the amount of light reaching the plate (or eye-piece) is proportional to the area of the aperture it is simple to work out the number of magnitudes or fractions of magnitudes which the various steps represent. Another method, which is the most accurate and the one most frequently used, is to place in front of the object glass a frame carrying a number of equally spaced parallel wires. The effect of this is to break up the ordinary image of the star in the telescope into a number of images : a central image flanked on either side by a series of images of diminishing brightness. The difference in the brightness of the successive images can be exactly calculated from a knowledge of the thickness of the wires and the distance between them. In this way with only a single exposure each star automatically gives rise to a scale of images representing known magnitude steps. The " coarse grating " as this frame of wires is called has had a varied career: it was invented by Schwarzschild for measuring the separation of double-stars in a way which need not concern us; it was adapted to the present use in 1901 by Prosper Henry of Paris; and it was subsequently to be used as we shall see for yet another purpose by Professor Hertzsprung.

In one or other of these ways a scale of known magnitude steps could be put on any plate and the relative photographic magnitudes of all the stars on that plate straightway determined. The next problem was how to link up several different plates—to fix the scale of a number of plates to the same arbitrary zero-point—so that the magnitudes of the stars on all of them should be mutually comparable. This had been solved in the *Göttingen Aktinometrie* by making the star field covered by each plate overlap the star fields covered by the plates adjacent to it. But the method was not entirely satisfactory. For anyone who has ever played the game of " Russian Scandal " will appreciate that the passing on of a standard from one plate to another must inevitably lead to a cumulative error.

A plan which was free from this objection was first proposed by Pickering and worked out in great detail at Harvard. The idea was to take the field of stars in the immediate vicinity of the north celestial pole, and to determine with great care, so to speak once and for all, the photographic magnitudes of the stars in it from the brightest to the faintest. The magnitudes of the stars in any other field could then be got by direct comparison with those of the polar field, by taking on the same plate two exposures in succession: one of the required field and one of the polar field. The polar field was selected for its obvious convenience: it could be reached by all the observatories in the northern hemisphere; and being fixed in the sky it would be available every hour of the night and all seasons of the year, and was always at the same height above the horizon. It would, of course, be essential that the exposure on the star field under investigation should be made when this field was at the same height above the horizon as the polar field so that the dimming effect of the atmosphere should be the same in the two cases. When the star field in question happened to be in a part of the heavens which never got so high in the sky as the pole, one would have to make use of some intermediary field. First the intermediary field and the polar field would be photographed on the same plate when at the same distance from the horizon, and secondly the intermediary field and the field for study would be photographed on a second plate when these were at the same distance from the horizon.

And so after many years of work in which " coarse gratings," adjustable diaphragms, graduated exposure times and various other methods were used, Pickering was able to give to the world in 1912 the Harvard " North Polar Sequence." It contains very accurately determined photographic magnitudes of ninety-six stars in the immediate vicinity of the pole, ranging from the 2nd to the 21st magnitude—a range in brightness of nearly forty millionfold. Since then the North Polar Sequence has been frequently revised, improved, and " filled in." In 1913 Professor Chapman and Mr. Melotte, of Greenwich, published the photographic magnitudes of 262 stars, which they had measured down to about magnitude 16 and within a distance of one diameter of the moon from the north pole. Finally Professor Seares of Mount Wilson combined together the results from Harvard, Greenwich, Potsdam and Mount Wilson to form a standard polar sequence which the International Astronomical Union accepted at its meeting in Rome in 1922.

We mentioned that visual and photographic magnitudes do not necessarily run parallel to one another. Whereas the eye, on which all visual magnitudes depend, is most sensitive to light of a yellow colour, the ordinary photographic plate is only sensitive to blue and violet light. In the case of two stars, a red one and a white one, which appear equally bright to the eye, the photographic plate will give for the red star, which is poor in blue and violet light, an image that is much smaller and less dense than that of the white star. This divergence of the two scales appeared at first sight to be a serious drawback to the use of the photographic method. But Schwarzschild soon realized that it could be turned to a great practical advantage. He saw that the difference between the photographic and visual magnitudes of a star must depend on the colour of the star; and he suggested that the figure which expressed this difference (photographic *minus* visual magnitude) should be defined as the *colour index* of a star. Since the stars in the *Göttingen Aktinometrie* for which he had measured the photographic magnitudes had all had their visual magnitudes determined for the *Potsdamer Photometrische Durchmusterung* their colour-indices were immediately available. It had long been realized that the colour of a star as roughly estimated by the eye ran parallel to its type of spectrum; but now, when the rough colour estimate was replaced by the much more precise colour-index, the parallelism was found to be extraordinarily exact. This important result was amply confirmed by similar investigations made by E. S. King at Harvard and by Parkhurst at the Yerkes Observatory. The immense value of this relation is at once obvious: we can photograph the *spectrum* of a star only if it is comparatively bright, but we can get the colour-index of the faintest star visible in the largest telescope, for it is always possible to get a *direct* photograph of any star. Furthermore as we shall see later the colour-index not only gives the spectral type of a star but also its surface temperature and indirectly its apparent diameter. Clearly therefore the importance of the colour-index of a star cannot easily be overestimated.

For his invention of colour-indices, his observations for the *Göttingen Aktinometrie*, and his development of precise methods in photographic photometry, astronomy owes to Schwarzschild a great debt. Yet probably his main contribution was his mathematical study of the movements of the stars and of the structure of the stellar system. To this we shall have to refer in due course, but only briefly because of its highly technical nature. Schwarzschild was undoubtedly one of the greatest astronomers of his day, yet he died at the early age of forty-six. During the first part of the war he served with the German Artillery and during this period contributed to the Berlin Academy an elaborate investigation on the effect of wind and air-pressure on the path of projectiles. It was the versatility of Schwarzschild that was his outstanding characteristic. His attitude is best summed up in his own words which at the same time express so clearly that new outlook in astronomy which has become so manifest in recent years. " Mathematics, physics, chemistry, astronomy, march in one front. Whichever lags behind is drawn after. Whichever hastens ahead helps on the others. The closest solidarity exists between astronomy and the whole circle of exact science. . . . From this aspect I may count it well that my interest has never been limited to the things beyond the moon, but has followed the threads which spin themselves from there to our sublunar knowledge; I have often been untrue to the heavens. . . . The motto runs that mathematics, physics, and astronomy constitute one knowledge, which, like the Greek culture, is only to be comprehended as a perfect whole."

The photographic like the visual magnitude system is quite arbitrary. As regards the visual system we saw that long ago its *scale* had been defined by Pogson's ratio, and that later its zero-point had been fixed by defining the magnitude of the Pole Star as 2·1. From the earliest days of photographic magnitudes the photographic scale had likewise been defined by Pogson's ratio, but for a long time there was no definite agreement as to its zero-point. At a meeting in Paris in 1909 of the Permanent Committee of the Astrographic Conference a number of people were appointed to consider the matter. Their recommendation adopted by international consent was to fix the zero-point of the photographic scale in such a way that the photographic and visual magnitudes would be the same for *white* stars of spectral type " Ao "—a classification explained on page 200. For white stars of this particular type the colour-index would be zero; for yellow, orange and red stars it would have an increasingly large *positive* value; while for the comparatively rare blue-white stars—stars " whiter " than those of type A—it would have a *negative* value.

The eye is well-known to be one of the most precise of photometric " instruments " but it has one serious weakness. So long as two stars are of approximately the same colour the eye aided by some form of photometer is able to make a very accurate comparison of their magnitudes. But as soon as there is any marked difference in their colour the comparison becomes difficult and uncertain. It is obviously unsatisfactory that one of the two observations upon which the measurement of the colour of a star depends—to wit, the estimation of the visual magnitude—should be most liable to uncertainty in just those stars in which the colour is most marked. It was Parkhurst and Jordan at the Yerkes Observatory who first showed in 1908 how this objection could be surmounted. It had recently become possible to make photographic plates which were sensitive to yellow light as well as to blue and violet light which alone affected the ordinary plate. By experimenting with various combinations of such plates and coloured filters they finally found a combination of which the resulting sensitivity was practically identical with that of the eye. In this way they were able to determine visual magnitudes by a purely photographic method. Such magnitudes are called " photo-visual " and are *ideally* the same as visual magnitudes, though even to this day their final standardization has not been completely defined. In recent years a great deal of work has been done in determining the photo-visual as well as the photographic magnitudes of the stars in the north polar sequence. Thus by a photographic comparison with the polar field we can find for any stars in the northern hemisphere both their photographic and photo-visual magnitudes, and hence their colour-indices, independently of visual observations.

We have seen how astronomers have set up a standard system of magnitudes—or rather two systems, a photographic and a visual or photo-visual—and how the magnitudes of any stars in the sky may be found in terms of those standards. We next turn to the study of the *variations* in the magnitudes of the stars, in fact to the study of variable stars. Here the methods used are in principle the same, but the object of the investigation is slightly different. In a variable star we are not primarily interested in its exact standard magnitude, its precise magnitude at any time in comparison with the Pole Star. We are mainly concerned with the fluctuations in its magnitude, their amount, and their duration.

The great majority of the brighter variable stars are those with long periods. Since they take months to go through their cycles, and usually change their brightness by several magnitudes, it is generally unnecessary to do more than measure their magnitudes by comparatively approximate methods from night to night. Moreover since there are thousands of these long-period variables, and since several hundred of them are kept under regular observation, it would be quite impossible to follow them all by the more accurate and laborious methods. Generally they are observed visually. The magnitudes of the stars in the same field as the variable are already known with accuracy; and each night the observer simply estimates which of these stars are equal to the variable, and which are brighter and which are fainter than the variable and by how much. In this way a skilled observer will in a few minutes get the brightness of the variable to within 0·1 magnitude. Sometimes for these stars an analogous photographic method is used. Photographs are taken night by night of the variable star and the stars around it. The photographic magnitudes of the surrounding stars are already known; and the observer simply by looking at the plate estimates which of these stars are equal to, brighter than, or fainter than the variable.

This photographic method has been used extensively at Harvard, particularly in the discovery of new variables. A large region of the sky is photographed on a small scale on two occasions separated by a period of several months, and the two plates are compared star by star. If there happens to be a long period variable in the region, its magnitude will probably be different on the two occasions and a corresponding difference will be noted in the images on the two plates.

Sometimes however, as in the case of the short-period variables—the so-called Cepheid variables and the eclipsing variables—it is important to study the way in which the light fluctuates with great accuracy. Either one studies the variation in its visual magnitude hour by hour or night by night, by using a wedge or polarizing photometer to compare its brightness with other stars near to it. Or one studies the variation in its photographic magnitude, by taking frequent plates and comparing its images with those of other stars by measuring their diameters or densities.

Eclipsing Variable Stars. We saw how probably long ago the star Algol had been caught winking by the Arabs; and how at the close of the eighteenth century Goodricke in England had discovered the perfect regularity of the wink and suggested that it was due to a periodic eclipse by a dark revolving star. In 1880 Pickering published the results of a very detailed photometric study he had made of this star, and showed beyond all doubt that the way in which Algol fluctuated was completely explained by assuming it to be periodically eclipsed by a dark star revolving round it. In addition he pointed out how careful measurements of brightness made throughout the light cycle of such stars enabled one to construct an exact diagram giving the relative dimensions of the system, that is to say the diameters of the two stars in relation to the distance separating their centres. Thus Algol was found to consist of a dark body with a diameter equal to three-quarters of that of the bright star round which it revolved; while the distance between the centres of the two stars was equal to about three times the diameter of the bright one. But the observations did not reveal the *scale* of the diagram, the dimensions of the system in miles.

We saw that in 1889 several apparently single stars had been revealed by the spectroscope to be double, and to consist of two suns revolving round their common centre of gravity. At certain times during their revolution their motions, which were in opposite directions, would be directed more or less towards and away from the earth; the spectral lines of the two stars would then be shifted in opposite directions and two separate spectra seen. At other times during their revolution their motions would be at right angles to the line joining them to the earth; and since neither of them would be then approaching or receding from us, their respective lines being unshifted would be precisely superposed and thus indistinguishable from those of a single spectrum.

Directly these spectroscopic double stars were discovered at Harvard, Vogel at Potsdam applied the same method to the study of Algol. He at once found that this star as well as being an eclipsing variable was also a spectroscopic double. Moreover the periodic shift in the spectral lines fitted in with the light fluctuations just as it should do according to the eclipse theory. Thus Vogel supplied a final convincing proof that stars like Algol were eclipsing double stars.

Algol however was not quite in the same category as the spectroscopic double stars discovered at Harvard: for it showed only a single spectrum. Instead, its single set of lines swung to and fro like a pendulum, first to the red and then to the violet, making it quite clear that the star was revolving in a circular orbit. And that, of course, was only possible if another star was concerned, another star which was either dark or so faint that its spectrum was invisible.

In succeeding years an enormous amount of work has been done in the study of these eclipsing variables and spectroscopic doubles. As might have been expected the eclipsing variables have all turned out to be spectroscopic double stars as well; while many spectroscopic doubles, not previously suspected of variability, have been found after careful measurements to undergo minute light fluctuations indicating a small partial eclipse. The occurrence of such an eclipse must clearly depend on the angle at which the orbit is tilted towards the earth. If we are looking at the orbit more or less edgewise an eclipse of considerable magnitude is bound to occur; while if we are looking down upon the orbit nearly perpendicularly, either no eclipse can occur or only a very small partial one.

By the most delicate of the methods so far described for measuring the magnitudes of the stars one cannot be absolutely certain of the result to within less than one-tenth of a magnitude—although of course one might often express it to one hundredth part of a magnitude. This means that if a dark star is revolving round a bright star in such a way that at each revolution it hides from our view one-tenth of the surface of the bright one, then the resulting eclipse would only just be detectable by the above methods. For at each eclipse the reduction in the brightness of the star would be ten per cent., and that is approximately equal to a diminution of 0·1 magnitude. Recently however two other methods have been used for comparing the magnitudes of the stars which are very much more accurate. These depend on certain electrical phenomena due to the action of light.

One of these, which makes use of a *selenium cell*, was first applied to astronomy in 1910 by Dr. Stebbins at the Observatory of the University of Illinois. The resistance of selenium to an electric current is modified by the intensity of the light falling upon it. A circuit is set up containing a battery, a selenium cell and a galvanometer. The cell is attached to the eye-end of a telescope and the light from a star is allowed to fall upon it for a few seconds. By comparing the deflections of the galvanometer needle produced by the light of different stars their relative brightness can be determined.

The accuracy attainable with the selenium photometer is far greater than that given by the best visual and photographic methods. With it the brightness of two stars can be compared to within less than 0·01 magnitude. But the use of the cell is fraught with technical difficulties: it is only to be used by an expert who is continually working with it, the observations are difficult and lengthy, and only the brighter stars come within reach of it.

The other electrical method depends on the use of a " photoelectric cell." The rate at which electrons are emitted from a surface coated with a thin film of an alkali metal, such as potassium, is dependent upon the intensity of the light falling upon the film and can be measured by means of an electrometer. The light from the two stars to be compared is allowed to fall in turn on the photo-electric cell just as was done in the case of the selenium photometer. The accuracy attainable with the photo-electric cell is even greater than that attainable with the selenium cell; but the technical difficulties in working it are also greater. The photo-electric cell was first applied to the study of variable stars about 1913 by Dr. Guthnick in Berlin, and Dr. Meyer and Dr. Rosenberg at Tübingen.

Observations made with these two forms of electric photometer have yielded results of the highest importance in the study of eclipsing double stars. Drs. Guthnick and Prager with their photo-electric photometer in Berlin have discovered minute fluctuations in the light of many spectroscopic double stars previously not suspected of variation. In some of these stars the fluctuation is less than 0·05 magnitudes, which corresponds with a partial eclipse in which the dark star covers from view less than five per cent. of the surface of the bright star. A discovery of special interest was made by Dr. Stebbins during a detailed investigation of the variation of Algol with his selenium photometer. It was known that once every three days—more exactly about sixty-nine hours—this star faded to about two-fifths of its normal brightness, losing and regaining its light in about twelve hours. It used to be thought that between successive fluctuations—in the intervals between successive eclipses—the light of Algol remained unchanged. Presumably, however, eclipses of the bright star by the dark star must alternate with eclipses of the dark star by the bright star; and if the so-called dark companion of Algol possessed any appreciable brightness one should be able to detect half-way between successive major fluctuations a fluctuation of minor degree. Now this is exactly what Dr. Stebbins succeeded in doing with his selenium photometer. Exactly mid-way between the primary eclipses of Algol he detected a secondary eclipse, in which the combined light of the two components diminished by only 0·06 magnitudes. Thus he proved that the companion of Algol was not a completely dark star.

But though the selenium and photo-electric cells are of such surpassing accuracy they possess numerous disadvantages. It is their very sensitiveness which makes them so difficult to cope with. A slight change in their temperature or a transitory haziness of the sky will be recorded equally with a real change in the brightness of a star. The most elaborate precautions must be taken and the observations checked and cross-checked repeatedly. Hence their use is confined to special studies of particular stars; and the stars most often observed are the eclipsing variables where accuracy and detail are of prime importance.

But why are astronomers so deeply interested in the eclipsing double-stars and the spectroscopic double-stars? In a spectroscopic double, by measuring the shifts of both sets of lines at various times during the cycle of their oscillation, the velocities of each star to or from the earth can be found in the corresponding parts of their orbits; and from these figures the precise shape of their orbits can be deduced. Now since one knows the time taken for the stars to go once round, and also the velocities with which they are going round, one might have thought that simple arithmetic would give the actual sizes of the orbits in miles. But unfortunately something is missing. The spectroscope measures only that part of the true velocity which is directed to or from the earth, only, so to speak, the true velocity more or less foreshortened. If we only knew the tilt of the orbit to the earth, how much the foreshortening was, then we could find the true velocity itself. Without that vital piece of information we can only say that the velocities are at least so great, and the orbits at least so large, though how much greater and larger they are we cannot tell. By Kepler's law it is simple to calculate from the period of revolution and the sizes of the orbits, the weights of each star—but in this case, for the reason just given, we can only say that each of them weighs *at least* so much.

The above argument applies only when both spectra are bright enough to be seen. In stars like Algol, where a single spectrum is visible, Kepler's law is unable to differentiate between the effects of the individual weights, and can only give us the combined weights of the two stars. But again it is only the least combined weight, for the tilt of the orbit is not known.

Now it so happens that the study of an eclipsing variable star puts into our hands that very piece of the puzzle which we required to complete it. For the diagram of the system, which we saw could be constructed from a study of the light fluctuations, includes that vital point, the angle at which the stars' orbits are tilted towards the earth. We are able, therefore, to obtain very intimate information about any spectroscopic double star in which both spectra are visible and which is also an eclipsing variable star. The light variation gives the tilt of the orbits; the tilt enables us to convert the least weights of the stars and the smallest sizes of the orbits, found from the spectroscope, into the actual weights and actual sizes; and finally the actual sizes of the orbits in miles supplies us with the scale to the diagram, and thus gives in miles the diameters of the two stars. For stars like Algol with only one spectrum visible the information is less complete: it gives simply the diameters of the stars in terms of their distances apart, and instead of the individual weights it only gives their combined weights.

Now until quite recently the only stars whose diameters could be actually measured were these eclipsing, spectroscopic doubles. Further a considerable proportion of the stars that allowed themselves to be accurately weighed also belonged to this class. That was why astronomers spent so much time in searching for these remarkable objects and in the very laborious investigation of them. They, naturally, wanted to get as much information as they could about the weights and sizes of the different types of stars.

The only other class of body outside the solar system that lent itself readily for purposes of weighing, was that of the more widely separated visual double-stars whose revolutions could be directly studied in the telescope. William Herschel had given the first concrete proof that the visual doubles were not always accidental —two stars just happening to lie in the same direction in the sky— by his observational demonstration of their orbital motion. And John Herschel, afterwards, had taken a leading part in showing that their orbits were described in accordance with Newtonian principles. Since then large numbers of visual doubles had been found to be revolving, and in many of them the orbits had been calculated. Their periods of revolution were very much longer than those of the spectroscopic and eclipsing doubles, and ranged (instead of between a few hours and a few weeks) from about five years up to hundreds or even thousands of years.

When one of these doubles has been watched over about a quarter of its orbital revolution the complete orbit can be determined. And then, if the stars are near enough for their distance from us to be measured, it is possible to deduce the size of the orbit in miles and get, from Kepler's law, the combined weight of the two stars. What is observed in the telescope is the " relative orbit," the orbit, that is, of the smaller star round the larger. In order to find the orbits of each of them round their common centre of gravity, it is necessary to measure, not only the motion of the stars relative to each other, but also the motion of one of the stars relative to the background of the sky. When that is possible, the individual weights of the two stars can be calculated. So out of the handful of visual double-stars for which the *combined* weights can be found, there is a still smaller handful in which the *individual* weights are known. By 1920 there were about sixty stars (thirty pairs), either visual doubles or eclipsing spectroscopic doubles, of which the individual weights had been found by such perfectly direct methods. Later on these were to form the nucleus around which were built up various *indirect* methods for getting the weights and other data for literally hundreds of stars.

CHAPTER VII

SOLAR PHYSICS AND THE MOTIONS OF THE STARS

Solar Physics. Now there is one star that is particularly well suited for detailed study; and that is our own private star, the sun. The sun is almost 300,000 times nearer to us than the nearest star; it is the only star close enough to show us its surface; and it sends us ten thousand million times as much light as Sirius, the brightest star in the sky. The stars differ from one another in brightness and colour, but otherwise they are identical in appearance, mere points; and even when we employ in their study the almost occult powers of the spectroscope we are handicapped by having to analyse the light from each of them wholesale. But the sun extends before us a broad panorama; and we can examine individually, by eye or by spectroscope, its poles or its equator, its centre or its edge, its general surface or its individual spots, or its different atmospheric appendages. Yet the most significant difference between the sun and the stars is that factor of ten thousand million which represents the ratio of sunlight to starlight. No star is so bright that its study is not handicapped by insufficiency of light, while the great majority of the stars in the sky are entirely excluded from spectroscopic study for this reason. The sun alone among the hosts of heaven is exempt from this drawback. And so it was that astronomers were not slow to realize that their best chance of learning about the stars was to start by tackling the sun.

In the closing years of the century a tremendous stimulus had been given to solar research by the invention, independently by Professor Hale in America and M. Deslandres in Paris, of that remarkable instrument the spectroheliograph. It enabled one to get pictures of the hydrogen sun, the calcium sun or the iron sun, portraying only those formations upon it that were composed of the particular vapour selected. Still more remarkable, it could photograph the sun's atmosphere at various levels: at its base, several thousand miles up near its summit, or at intermediate heights.

The discovery of the method of viewing the prominences, which are visible round the sun's edge during a total eclipse, without having to wait for such an event, had naturally resulted in their becoming the subject of routine investigation. They were soon found to consist of two sorts: the quiescent type that lay quietly on top of the chromosphere and had little to do with sun-spots; and the eruptive type that underwent incredibly violent changes and generally made their appearance over a part of the surface in the close vicinity of a spot. The eruptive type would be seen leaping from the underlying chromosphere with velocities that sometimes reached several hundred miles a second, and sometimes it was actually possible to see them moving while one watched. Once Young in America saw such an object, 100,000 miles long and 50,000 miles high, blown to pieces and utterly demolished within less than half an hour. Moreover, the shift in the spectral lines of these objects showed that their horizontal velocities parallel to the sun's surface were often as great as those in the vertical direction.

But what was the nature of the black spots underlying these celestial rockets? It was thought at one time that they represented holes, rent by some underlying violence in the surface, through which we looked into the darker interior. Later it was found that the spots were at a lower temperature than the rest of the surface. Lockyer showed that, in passing from the general surface to the spot, the spectrum was modified in the same way as the spectra of corresponding terrestrial substances were modified in passing from their examination in the electric spark to their examination in the electric arc. It was known that vapours in the spark reached a higher temperature than in the arc; so presumably the ordinary solar spectrum indicated a higher temperature than the sun-spot spectrum.

Spots are often double, and it is very common to see two large spots linked together by a chain of smaller ones. Quite an ordinary sized spot would engulf the earth many times, while the area of the sun's surface covered by a single group of spots frequently totals several thousand million square miles. Yet the limits of the disturbance are by no means confined within the spot-group; for extending to great distances all round are numerous dazzling white flecks coalescing here and there in patches. It seemed almost that these brilliant flecks—the faculae, so-called—were more fundamental than the spots: they usually preceded the development of the spots, and they generally lingered on after the spots had disappeared; what was more, the prominences seemed to be associated not so much with the spots as with the surrounding faculae.

The study of the faculae made rapid strides so soon as the new spectroheliograph of Professor Hale and M. Deslandres, referred to above, was perfected. Put briefly, this instrument enabled one to select from any part of the spectrum just one line, and use the light in that line—to the exclusion of all other light—for the photograph. It may sound odd to talk about the *light* in a *dark* spectral line; but the lines in the solar spectrum only appear black by contrast, and are by no means absolutely dark. The spectroheliograph picture of the sun, if taken with the light in one of the hydrogen lines, portrays the clouds of hydrogen in the solar atmosphere; if taken with the light in one of the calcium lines, it portrays the clouds of calcium, and so on. And if one examines these various pictures one finds that the various kinds of clouds are very different from each other. M. Deslandres went a step further. The calcium lines are quite broad bands; and he found that while light from their edges depicted calcium clouds at the bottom of the sun's atmosphere, light from their centres showed clouds at the highest levels, and light from their intermediate parts clouds from intermediate levels. Thus by sliding the slit of the instrument from one part of the band to another one was able to obtain photographs of the different layers of the sun's atmosphere.

It soon became clear that the lowest level calcium clouds photographed in this way were the faculae which were visible in the ordinary telescope. The higher level calcium clouds, though preserving a general similarity, were much more extensive and often completely covered over the underlying spots. The hydrogen clouds, however, were quite different in form. Moreover the hydrogen photographs, which portrayed the highest levels of all, showed, in addition, black curved markings which turned out to be the prominences seen in silhouette against the surface. Previously the prominences could only be seen as they projected from the edge; henceforth their behaviour could be watched during the entire thirteen days that the sun's rotation took to carry them across the disc from one edge to the other. The spectroheliograph achieved an exactly similar advantage for the study of the faculae. With the telescope these bright flecks can only be seen fairly close to the sun's edge; directly they get near the centre of its disc, which is so much brighter than the edge, they become invisible through lack of contrast. But with the spectroheliograph they can readily be followed from one side of the disc to the other.

The science of solar " meteorology " was now able to begin in real earnest. One of the first things to emerge from it was the cyclonic nature of sun-spots. The black hydrogen filaments, or prominences, were sometimes seen to be arranged around the spots like the whorls of a vast maelstrom. And on one occasion Professor Hale obtained a remarkable series of photographs showing a prominence being drawn in towards a spot and sucked down until it disappeared.

It was soon found that the spectroheliographs and spectroscopes that could be used with advantage on the sun were far too massive to be attached to an ordinary telescope. So it became the custom to use a stationary telescope fixed horizontally upon the ground, the sunlight being fed into its lens by large, optically perfect, and delicately moving mirrors. In this way a telescope of great length and magnifying power could be employed and the light from it examined with spectroscopes capable of drawing out the solar spectrum to enormous lengths. Still more important, the entire outfit could be housed in this way in a perfectly insulated building from which all disturbances due to irregularities of temperature could be excluded.

In 1904 there was founded in California the forerunner of the present Mount Wilson Observatory. It was then the Mount Wilson Solar Observatory and its interests were officially confined to the sun. The observatory, which is at a height of 5,868 feet, was erected under the supervision of Professor Hale. With him went also Professor Adams (the present director), Barnard and Professor Ritchey; and the presence of these two latter astronomers gave promise that though the main object of the observatory might be to tackle the problems of solar physics, it would not be long before it enlarged its scope to include also those of general astrophysics. In fact Barnard had taken with him his Milky Way camera, and it was already rumoured that so soon as the trail up the mountain had been sufficiently widened Professor Ritchey would haul up it the 60-inch reflecting telescope he had already almost completed. But so far the " Snow Solar Telescope," was the only considerable piece of apparatus. This was really two telescopes mounted horizontally side by side, one 60 feet, the other 145 feet long, into the mouths of which the sunlight was fed by rotating mirrors. Very soon afterwards another experiment was tried: a telescope was built, immovable and fed by mirrors like the last, but instead of being horizontal it was mounted vertically like a tower. The lens and moving mirrors were at the top of the tower, while the spectroscopes, etc., were sunk in a constant temperature chamber deeply beneath the ground. The first tower telescope was 60-feet high; and it was soon followed by another 150-feet high, at the bottom of which a spectroscope, a further 75-feet in length, was placed.

This stupendous equipment, coupled with the originality and skill of Professor Hale, resulted in 1908 in the important discovery of the magnetic field surrounding the sun-spots. When an electric spark is placed between the poles of a powerful magnet the spectrum emitted by it is modified in a characteristic manner. Professor Hale was able to detect an exactly similar modification in the spectrum of the spots. When as so often happened the spots occurred in pairs, they would invariably have an opposite polarity—one corresponding to the positive and the other to the negative pole of a magnet. Moreover all the western spots (of the pairs) in the northern hemisphere of the sun had the *same* polarity; and while the same rule held in the southern hemisphere, the polarity of the pairs in the two hemispheres was the *reverse* of each other. In 1912 when the old sun-spot cycle died out at the equator and the new one started in high latitudes, Professor Hale found that the polarity of the two hemispheres, so to speak, changed over: the western spot of one hemisphere taking over the polarity which had been the prerogative of the western spot in the opposite hemisphere during the preceding eleven-year cycle.

This magnetic field surrounding the spots was best explained as the result of vigorous currents of gases (carrying electrified particles) rising or falling over the spots. And this view fitted in very beautifully with another remarkable observation. When the cyclonic motion of both members of a pair of spots could be followed with the spectroheliograph, they were found to be rotating in *opposite directions* in correspondence with their *opposite polarities*. But probably the most valuable piece of evidence in favour of the cyclonic theory of spots was the discovery in 1908 at Kodaikanal of the "Evershed effect." Mr. Evershed found that the gases always flowed outwards from the spots—overflowing, as it were, on to the sun's surface; and further, the speed of the current was greatest for gases at the deepest levels. This beautiful and excessively delicate observation was confirmed in 1913 at Mount Wilson by St. John, who found further that at higher levels the direction of flow was reversed the gases moving inwards towards the sun-spots. Yet another important discovery must be referred to in this connexion. In 1908 Professor A. Fowler, of London University, identified in the spot spectrum numerous lines due to titanium oxide. Now the temperature of the ordinary surface of the sun is far too high to allow these elements to combine into compounds, so this discovery provided additional proof that the temperature of the spots was considerably lower than that of the general surface.

All these discoveries have been embodied by Professor Hale in his theory of sun-spots. The two members of a pair of spots represent the open mouths of a deep U-shaped vortex opening on to the surface. In the two arms of the vortex the gases rise spirally upwards, emerging from the spot in precisely the opposite manner (as Professor Russell remarks) to water running out of a wash-basin. The hot gases suddenly expand on reaching the surface and undergo a tremendous fall in temperature; and the mass of cold gas by absorbing the light from below produces the black spot we see. A condition analogous to our own anti-cyclones is now set up in the atmosphere overlying the spot: the gases below flow outwards over the sun's surface (Evershed effect), and those above flow inwards as shown by St. John.

Stellar Motions. In earlier parts of this book we have followed the discoveries relating to certain physical features of the sun and stars. We must now turn to the question of their movements through space; for during the first ten years of the present century a number of events occurred which at last began to give to this problem a much more definite form. In 1718 Halley had been the first man to challenge the epithet " fixed " as applied to the stars. He had shown that a few of the brighter stars had unquestionably shifted their places in the sky during the fifteen centuries that had elapsed since Ptolemy had recorded their positions; and within the next fifty years the number of known " moving stars " had grown to over sixty. Towards the end of the same century the elder Herschel had shown that part of the stellar movements were due to our being transported with the sun and the rest of the solar system bodily through space: that ahead of us the stars were opening out and behind us they were closing in, much as the trees do in a wood when we are walking through it. The next step had been the revolution wrought by Bessel in the methods of " reducing " measurements of star-positions. He had evolved a systematic scheme by which certain spurious effects could be allowed for: effects due to instrumental and observational errors, effects due to the atmosphere and to the non-instantaneous velocity of light, and effects due to the wobbling and nodding movements of the earth's axis. Henceforth star-positions had been far more reliable and far freer from ambiguity. By applying his scheme to the old observations of Bradley at Greenwich, he so to speak antedated the epoch of reliable measurements by half a century, and gave astronomers a trustworthy catalogue of star positions for 1755, with which future positions could be compared for the study of stellar movements. He had moreover taken his last step himself, and by comparing the Bradley observations with those of his own time, up to about 1810, had greatly increased the number of well-ascertained stellar motions.

The stellar motions here referred to are what are known as proper motions: the motions left over when all spurious motions due to variations in the tilt of the earth's axis and to the velocity of light have been deducted from the motions directly observed. The proper motion is the star's motion in a direction at right angles to the line joining it to the earth, its motion upon the celestial sphere; it is the motion which can be detected directly with the telescope if only enough time is allowed for it to become appreciable. Proper motion thus clearly consists of two parts: the star's own "peculiar motion," and the apparent motion impressed upon it by our own motion as we are carried along with the solar system in its journey through space. We distinguish between these two parts just as, from the window of a moving railway carriage we distinguish between the real movements of men and cows walking, and the apparent movements of spires and chimneys against the background of more distant hills. In interstellar space all movements are relative, and there is no direct way of apportioning fairly a given proper motion between the star and solar system. The best we can do is to take all the available proper motions of stars from all over the sky, find the average of them, and consider this as the speed and direction of the stellar system relative to the solar system, or, what comes to the same thing, of the solar system relative to the stellar system.

During the last century great strides had been made in this investigation. Many new catalogues of star-positions had been published and compared with Bradley's positions for 1755. More numerous and more accurate determinations of stellar motions had thus gradually accumulated, and these in their turn had yielded more precise information on the sun's march through space. The direction of the solar motion which they indicated tended to agree closely with that first pointed out by Herschel. But the velocity of march, which Herschel had been unable even to guess at, was a very much more questionable matter. To find the velocity necessitated measuring not only the proper motions but also the distances of the stars; and while many of the stars had measurable proper motions very few of them were near enough for their distances to be accurately determined.

It was at this juncture that the ever-helpful spectroscope sprang to the rescue, with its ability to measure the velocity of the stars in a direction directly towards or away from the earth. The motion which the spectroscope measured was, of course, precisely that part of the motion which was *not* the proper motion. The proper motion is the motion of the star at right angles to the line joining the star and the earth, and it causes the star gradually to change its position in the sky: the longer the interval the more it accumulates and the more readily can it be measured. The motion measured by the spectroscope is actually along the line joining the star and the earth; for this reason we talk about the " radial velocity " of the star since it is the velocity radially outward from, or radially inward towards, the earth. Moreover the spectroscope measures the radial velocity immediately, with an accuracy depending only on the powerfulness of the spectroscope: there is nothing to be gained by waiting, as there is in the case of proper motions. One other important distinction is that whereas the spectroscope gives the radial velocity straightway in miles per second, the telescope only gives the proper motion in *fractions of a degree per year*, and this can only be converted into miles per second when the distance of the star has been measured. By taking the radial velocities, as we did the proper motions, of all available stars all over the sky, and again by averaging them out, we should arrive not only at the direction but also at the speed of the sun's motion through space. Directly ahead of us the average radial velocity of the stars should show a certain rate of approach; directly behind us the average radial velocity should give a similar rate of recession; and in regions of the sky half-way between these two directions the radial velocities should cancel out, their average being zero. This was precisely what Professor Campbell found from the radial velocities of 280 stars measured at the Lick Observatory in California. The preliminary result which he published in 1901 gave a velocity of 12½ miles per second for the sun's motion through space. The direction he found for it, however, was only in fair agreement with that given by the proper motions; but that was considered not to be surprising: for the data available to him were somewhat lop-sided, since there was no telescope south of the equator powerful enough to measure the radial velocities of the southern stars.

The next important event in the study of stellar motions was the publication in 1904 of a *Standard Catalogue* of the proper motions of over 600 stars, by Lewis Boss, the director of the Dudley Observatory, Albany, N.Y. It was based partly on Boss' own observations but mainly on an elaborate study of all existing catalogues, some seventy in all. There are bound to be in every star catalogue minute idiosyncrasies, minute systematic errors affecting the positions given in one catalogue slightly differently from those given in another catalogue. Consequently no catalogue is strictly comparable with any other, until the individual idiosyncrasies are discovered and allowed for and the various catalogues thus brought into line with one another. Although the systematic errors will be so small as not to matter in regard to the proper motion of any particular star, their existence might very appreciably affect the average proper motion of many stars derived from several different catalogues. The work of minutely analysing some seventy catalogues and thus rendering them homogeneous occupied many years. To some of us the task might appear tedious and even a little dull, and we might almost have sympathized with that Trustee of the Dudley Observatory who asked why the Director did not occupy himself with the more exciting pastime of searching for comets. Boss actually accepted the challenge and took a shot at an alarming range: if money for an assistant were provided, then a comet should surely be discovered. What was more, the shot took effect, and the great comet of 1882, one of the brightest of the century, fell to the bag of the Dudley Observatory! Thus Boss won freedom for his work and was able to give astronomers the first trustworthy list of proper motions from the north to the south pole. In 1910 he revised this list and enlarged it tenfold, to include over 6,000 stars; and up to the present time Boss' Preliminary General Catalogue is regarded as giving the largest and most homogeneous series of proper motions in existence.

So soon as the study of stellar motions began to be planted on a firm basis the question naturally arose as to whether the motions were entirely haphazard or were governed by any rules and regulations. In 1904 Kapteyn at Groningen made a most significant discovery. He found that the previously discovered tendency for the stars to stream in one direction required modification, and that actually they were streaming in one of *two* favoured directions. A tendency to stream in one direction was, as we explained above, the necessary consequence of the motion of ourselves with the sun and planets through the stellar system. But streaming in two directions could only mean that in addition a part of the stellar system was moving relatively to another part of it. The previous investigators who were simply in search of the solar motion, had missed the double nature of the stream because they had only considered the average proper motion of the stars as a whole. Kapteyn went into the matter in more detail and studied the motions of the *individual* stars. He found the stars with measurable proper motions could be divided into two groups: one consisting of three-fifths of the stars, and one consisting of two-fifths of the stars, the two moving in opposite directions to each other. The direction of the sun's motion was inclined at a considerable angle to the line along which in one direction or the other the two star streams moved. Of course every star did not travel exactly in one or other of these two directions; their movements were in all directions, but there was a general tendency to move to or fro along this line. An analogous state of affairs is found in transatlantic shipping. Boats on the Atlantic sail in directions corresponding to all points of the compass, but they can clearly be regarded as belonging to one of two main streams, an easterly and a westerly going stream. If we happened to be on a boat sailing from Liverpool to Buenos Ayres we should obviously belong to the westerly going stream although we should have a direction (south-west) considerably deviating from the westerly direction of our stream. The larger of the two star streams is directed towards a point in the constellation of Orion, while the smaller is travelling towards the opposite part of the sky, towards a point in the constellation of Scutum. But the most remarkable thing of all is that the line joining these two points is exactly parallel to the plane of the Milky Way. And that immediately suggests that star-streaming represents some very fundamental property of our sidereal system. One idea was that it was a purely local phenomenon produced by two of the star-clusters of our stellar system, to one of which we belonged, passing by chance through one another. But if so it was a little odd that the "visiting team" had just happened to run into us in a direction exactly parallel to the Milky Way, parallel that is to the plane of the disc-like sidereal universe.

A more plausible explanation put forward by H. H. Turner gave to star-streaming a much grander significance. Was it not the manifestation of the gravitational attraction of the whole sidereal universe compelling each star to pursue an elongated orbit round the universal hub? These orbits would naturally be most numerous in the plane of the Milky Way; and viewed from a point some distance from the centre, as the earth probably was, they would give rise to apparent motions tending to be to or fro along a line directed towards the galactic centre.

A remarkable modification of this theory was discussed by Sir Arthur Eddington about 1913. The idea had long been toyed with that the spiral nebulae, which the camera had shown to exist in such vast numbers, were systems external to the sidereal universe. If the spiral nebulae were outside the stellar universe, might not each one in itself be an individual stellar universe, or, what comes to the same thing, might not our own stellar universe be in fact a spiral nebula? Many of the spiral nebulae were turned edgewise to the earth, and these were seen to be, like the Milky Way, very thin in comparison with their diameters, rather like a biscuit. Moreover photographs of these edgewise nebulae showed them often to be girt about their waists by dark obscuring matter analogous to the obscuring material that Barnard's photographs had shown to be so prolific among the star-clouds of the Milky Way. It was thus quite probable that to an observer somewhere within a spiral nebula the view presented would be not unlike that of the Milky Way seen from the earth. As Sir Arthur Eddington pointed out at that time, although it was not known whether the stars in a spiral nebula were travelling inwards or outwards along the spiral arms, it was extremely probable that they were doing either the one thing or the other. Since all spiral nebulae showed *two* arms originating from the *opposite* sides of a central nucleus, it was clear that in their central parts there must be two oppositely directed streams of stars. Thus was it not possible that Kapteyn's two star streams represented the pouring of stars into, or out of, the roots of a vast spiral, thus bearing testimony to the validity of the scheme in which the entire stellar universe—including all stars to the faintest and most distant in the clouds of the Milky Way—was but one among countless thousands of similar systems?

Meanwhile the tedious labours of Lewis Boss, in the preparation of his great catalogue of Proper Motions, had been rewarded by a discovery even more tangibly dramatic than his mercenary capture of the famous comet of 1882. In 1908 he found that the Hyades—a striking V-shaped group of stars forming the horns of the Bull in the constellation Taurus—were travelling together through the sky. But most important of all, these stars, instead of moving exactly parallel to each other, were all converging precisely on a certain point. Now there is no earthly reason why a group of stars should actually be moving in a converging stream, and there is a very good reason why they should be moving exactly parallel to one another. Undoubtedly, therefore, the convergence was only apparent and due to perspective: it was like the apparent convergence of parallel railway-lines extending into the distance, or the converging motion of a group of aeroplanes which have passed overhead and are retreating towards the horizon. Now it could not be a pure coincidence that a large number of stars in a limited region of the sky were travelling together through space in the same direction and at the same rate. They must clearly be members of a physically related family and boasting in some sense a common origin. Whatever that origin was it was clear that they had been launched on their sidereal careers in a condition of *rest* relative to each other, though possessing a common motion with respect to the rest of the stellar system. If that were so they would pursue for ever mutually parallel paths except in so far as they were disturbed by their gravitational attractions upon each other and by the attractions of other stars which they chanced to encounter. Such effects were, however, likely to be inappreciable; for though the stars in the Hyades were packed more closely than the general packing of stars in space they were nevertheless separated from each other by very great distances.

The idea of physically connected groups of stars was not a new one. The telescope had long ago revealed numerous condensed star clusters, hundreds of stars packed together in one small region of the sky so closely that even the most powerful instruments could barely separate them. But such conglomerations were obviously at great distances, and it was by no means clear that the stars in them were really so closely packed: they might merely be outlying parts of the stellar system only apparently condensed by reason of their vast distances. But the Hyades, being so much nearer home, afforded the first opportunity for studying the motions of the individual stars in a cluster, and this had removed any doubt that remained as to the existence between such stars of a definite physical connection.

But the most important part of Boss' discovery was the fact that the motion of the Hyades was *convergent*. He showed that it immediately gave one a means of determining very accurately the distance from the earth of each star in the group. Having found the point in the sky towards which the stars were converging, and having measured with the spectroscope the velocity with which a few of the stars were receding from the earth, a very simple geometrical solution gave the distances from us of every one of them. The Hyades are too far away for their distances to be measured accurately by the ordinary method of the apparent yearly shift; yet the new method gave their distances as accurately as the ordinary method could give the distances of the nearest stars in the sky. The average distance of the stars in the Hyades was found to be such that light takes 120 years to traverse it. It is a roughly spherical group which light takes thirty years to cross from one side to the other. Knowing their distances we can work out the real brightness of the various stars; and no less than thirty-four of them are found to be between ten and a hundred times as bright as the sun. This means that the stars in the Hyades are much more crowded together than the stars in our own neighbourhood; for within a sphere of similar size (thirty light-years across) surrounding the sun there are only three stars of that order of brightness.

The recognition of other " moving clusters " quickly followed; one comprising a considerable portion of the constellation Perseus, another covering an extensive part of the sky in Scorpio and Centaurus, and a third including most of the bright stars in Orion. But the most interesting of all was that discovered by Professor Hertzsprung in 1909, which claimed among its members stars from all over the sky. In other words, the Hertzsprung cluster was all around us, actually enveloping us. The sun, however, was not a member of it, only a chance interloper just happening to be passing through it. Five of the brightest stars in the Great Bear and the dazzling Sirius itself belonged to this cluster. Now Sirius is near enough to us for its distance to be measured by the ordinary direct method with considerable accuracy; and so there was an excellent opportunity for checking up on the reliability of Boss' indirect method for getting the distance of a converging cluster, which could, of course, be applied to the Hertzsprung cluster as it had been to the Hyades. One must realize, however, that this method worked only when the converging point could be precisely determined; when, that is to say, the distance of the cluster was not too great. It gave excellent results for the Hyades and the Hertzsprung cluster, but less good results for the Perseus, Orion and Scorpio-Centaurus clusters. In still more distant groups like the naked-eye cluster, the Pleiades, the motions were found to be equal and parallel, but were too small to show any appreciable convergence; so the method for range-finding was of no use. Finally, in the ordinary telescopic cluster the distances were so vast that no motions could be detected at all.

The recognition of moving clusters represented a big step forward. First, it extended our range-finding methods to include a large group of stars previously beyond their limits; and incidentally it enabled us to find the actual brightness of these same stars. And secondly, it showed that the tendency to exist and travel together in clusters was a common trait among the stars. It seemed as if the sidereal universe was a sort of hierarchy of stellar families, each one preserving its independence as its members journeyed together onwards through space.

Although the direct method of measuring a star's distance and the less direct method of gauging the distance of a " moving cluster " carried us a long way outside the solar system, they obviously did not carry us very far into the system of the stars. There was no method of getting by observation the distances of even the nearer star-clouds of the Milky Way, let alone of determining the extent of its furthest boundaries. Yet these were problems of prime interest to astronomers, and for some time past considerable progress had been made in their investigation by methods which were not directly observational but statistical.

The originator of the statistical method in its most elementary form had been William Herschel. By counting the number of stars visible in his telescopes in different parts of the sky he found that the stellar system, so far from having an infinite extent, was of quite definite dimensions. He found that it had the shape of a flattened disc and reached about five times as far in the direction of the Milky Way as in the direction perpendicular to the Milky Way. Although this conclusion is in remarkably close agreement with the results of modern investigations, many of the assumptions upon which Herschel based his arguments have had to be modified in the light of recent discoveries. Thus Herschel assumed that the candle-power, or the absolute brightness of the stars did not vary very much from one star to another, and that the apparent brightness of a star in the sky could therefore be taken as a rough index of its distance. We know now that the variation in candle-power from one star to another may be enormous.

It was not until about fifty years after Herschel's death that another champion of the statistical method arose. This was Hugo von Seeliger, the Director of the Munich Observatory, who between 1884 and 1911 revived the method and put it upon a much more comprehensive basis. Seeliger was fully aware that there were enormous differences in the absolute brightness of the stars, and that this necessarily complicated the problem of determining the relative extent of the stellar system in different directions. He was a very able mathematician and was able to show that this difficulty could be overcome if one counted in different parts of the sky not only the total number of stars visible, but also the number of stars between different ranges of apparent brightness. Theoretically it was possible from these counts alone to determine the rate at which the stars fell off in number at increasing distances from the centre of the stellar system. This naturally constituted a great advance on Herschel's more primitive type of analysis which had taken no account of the tremendous differences in the absolute brightness, or candle-power, of the stars.

So far the statistical methods employed gave only the *relative* rates at which the stars are beginning to thin out in different directions in the stellar system; they gave one no idea as to the actual distances of the boundaries of the system.

Now in the direct method of determining a star's distance one measures the small amount by which it appears to shift its position in the sky as a result of the earth's orbital movement round the sun. This annual apparent shift of a star to and fro in the sky may be regarded as the reflection of the earth's motion round the sun. Theoretically it affects every star in the sky, but since it gets smaller and smaller the further away a star is, it is far too small to be detected except in the case of stars that are comparatively close to us. In addition to the annual motion of the earth in a circle round the sun there is the motion of the whole solar system through space. This motion is in a straight line, and as a result of it the earth is carried forwards each year in a straight line over a distance equal to about twice the diameter of its path round the sun. We saw how this motion also produces a shift in the positions of the stars in the sky. But whereas the orbital motion gives rise to an apparent oscillation of the star in the sky to and fro each year, the motion of the solar system through space makes the star appear to move continuously in a straight line. It might have been supposed that this second type of shift—which is known as the " parallactic motion " of a star and is the reflection so to speak of the motion of the solar system through space—might likewise be used for determining the distance of a star. But unfortunately this cannot be done; for it is impossible to disentangle this *apparent* part of a star's motion, its parallactic motion, from the *real* part which is due to the movement of the star itself and is known as the " peculiar motion."

But although we cannot find the parallactic motions and hence the distances of individual stars, we can find the *average* parallactic motions and hence the *average* distances of large groups of stars. We saw that the stars like the sun are moving about in space with all sorts of different velocities and in all sorts of different directions. If we select at random a large enough group of stars their real, or peculiar, motions will cancel out and leave us only with that motion which is common to them all, namely, their *apparent* or parallactic motion. Of course the problem is not quite so simple as this. In the first place one has to make certain that one has really selected the group of stars at random, and that one's choice has not been unconsciously biased by some factor which directly affects the question at issue. And in the second place one knows that the individual movements of the stars are not entirely at random, so that due allowance must be made for that. However such things merely make the problem more complicated and add, perhaps, to the fascination of its disentanglement.

It was by this method that Kapteyn of Groningen, from a study of the " proper motions " of a great number of stars, worked out the average parallactic motions and hence the average distances of different groups of stars lying between certain limits of apparent brightness. In fact he worked out a formula which gave one the average distance of all the stars having any particular apparent brightness. This formula was published in 1901 and was afterwards employed by Seeliger to supply the actual distances in his own investigation based on star counts. In this way Seeliger was able to derive the first satisfactory estimate of the dimensions of the stellar system. He found its diameter—the diameter of the Milky Way—to be 23,000 light-years, and its thickness, measured at right angles to the direction of the Milky Way, to be 6,000 light-years.

The methods of Seeliger were further improved and refined by Schwarzschild, the Director of the Observatory at Göttingen. He not only contributed largely towards the solution of the general mathematical problem, but also investigated the way in which the distribution of the stars differed from one spectral type to another.

But the greatest exponent of the statistical method as applied to the study of the stellar system was undoubtedly Kapteyn himself. The name of Kapteyn will live among the greatest in astronomy. He has added so much to our knowledge of the structure of the sidereal universe that we must not pass on without referring briefly to the various phases of his work. In 1878, at the age of twenty-seven, Kapteyn was elected to the newly-instituted chair of Astronomy at the university of Groningen. Since the university possessed no observatory his official duties were confined to lecturing. On hearing of Gill's intention to make at the Cape a photographic survey and catalogue of the stars in the southern hemisphere, he wrote and offered to undertake the arduous task of measuring and reducing all the plates. This took him twelve years and resulted in the publication in 1900 of the great Cape catalogue already mentioned which gave the positions and brightness of over 454,000 southern stars. It speaks highly for the enthusiasm and enterprise of Kapteyn that this stupendous labour was carried out in two small rooms borrowed from the physiological department of the university, with a measuring machine designed by himself, and practically single-handed. He started with the help of one man whom he trained himself, and at one time employed a few convicts loaned to him by the authorities of a Dutch prison.

From the investigation of star-positions he proceeded to the study of stellar distances. In 1889 he proposed a plan for determining from photographic plates the distances of stars wholesale. The plates were taken at Helsingfors and again the measurements were made by Kapteyn; and within a few years he had published specimen results for several thousand stars. His next step was from the distances to the motions of the stars; and it was in this stage of his work that he published the statistical formula we have just discussed connecting the average distances of the stars with their apparent brightness. At the same time he derived a second formula of statistical importance. It will be remembered that when Bessel was looking for a star that was near enough to us for its distance to be measured by the direct method, he selected not the brightest stars but rather those which showed the most rapid motion across the sky. Although the stars do not all have the same velocity in space, their velocities are confined within fairly narrow limits. If a star is changing its position very slowly in the sky there are three possible explanations: (1) it is a long way away, (2) its real motion through space is slow, or (3) the direction of its motion just happens to be more or less directly away from the earth, so that whatever motion it has produces very little change in its position in the sky. But if we take a large group of stars apparently moving very rapidly in the sky, another large group moving less rapidly, and still other large groups moving still less rapidly, the real movements of the stars and their directions will average out the same and we should expect to find that the groups with the most rapid motions in the sky will be those which are on the average nearest to us. So Kapteyn took all the stars of which the distances had been directly measured and derived a purely empirical formula connecting up their proper motions, the rate at which they were apparently moving across the sky, with their distances from the earth. With the aid of this formula, and that other formula of Kapteyn which correlated the distances with the apparent brightness of the stars, it became possible to give with a considerable degree of certainty the average distance of a group of stars of which the brightness lay within certain limits and of which the proper motions lay within certain limits.

It was during this investigation of the motions of the stars that Kapteyn was led in 1904 to his great discovery that the majority of the nearer stars tended to move in one of two favoured directions in the sky. This phenomenon had naturally to be taken into account in all future statistical investigations; for clearly if the stars tended to move in one of two favoured directions their motions could not be regarded as entirely at random.

One of the main difficulties of these statistical investigations is that much of the data is only available for the brighter stars and cannot therefore be regarded with certainty as providing a "perfectly fair sample." The reason is that it is only with considerable trouble and often with special instruments that many of the researches can be extended to the fainter stars. And so it was that Kapteyn proposed his well-known scheme of "selected areas." 252 small regions of the sky distributed more or less uniformly over the heavens were chosen, and various observatories all over the world were asked to co-operate in studying the positions, brightness, distances, motions, radial-velocities and spectral types of all the stars down to the faintest that could be reached within these regions. Although this scheme is still far from completion it had, even in Kapteyn's lifetime, accomplished much towards filling up the gaps in our knowledge. Kapteyn's final conclusions as to the dimensions of our stellar system made them very much larger than those given by Seeliger. Seeliger had put the diameter at 23,000 and the thickness at 6,000 light years respectively; Kapteyn increased the diameter to 55,000 and the thickness to 11,000 light years. Of course neither of these investigators considered that the system had an abrupt end and possessed sharply defined boundaries. They had shown that the stars gradually thin out as the trees do upon the edge of a forest, and they gave as their definition of the boundaries of the system those regions where the number of stars in a given volume of space was one-hundredth part of the number of stars in the same volume at the centre of the system.

Kapteyn died in 1922 after fifty years of continuous work. His far-sightedness and his ability as an originator and organizer of big schemes, his skill and untiring energy in the measurements and reduction of photographic plates, and finally his able discussion of the results, brought about an immense advance in our knowledge of the structure of the stellar system. And perhaps of still greater importance was the stimulus which his suggestions gave to co-operative work in astronomical observation. The seeds which he sowed in this way will continue to bear fruit for a long time; for so long as telescopes continue to increase in size, so long will the "plan of selected areas" continue to be pushed to ever fainter limits among the stars. In the last few years of Kapteyn's life an entirely new method was being developed for determining the distances of the boundaries of the stellar system. This was a return to the more or less direct methods of observation; and we shall see in Chapter IX to what extent it was to confirm and to what extent to modify the statistical results we have just been considering.

CHAPTER VIII

WIRELESS TIME-SIGNALS, STELLAR EVOLUTION AND RELATIVITY

Wireless Time Signals. During the year 1913 a large number of observatories installed as part of their equipment a wireless receiving station for getting the time signals from the Eiffel Tower and from Norddeich-Wilhelmshaven. This was really the start of wireless time signals for routine astronomical use. In 1905 the United States Naval Department had started sending out time signals for the use of shipping, and a similar service had been started in France and Germany in 1910. At the instigation of Poincaré, the French mathematician, a conference had been called in Paris in 1912 to organize an international scheme for covering the globe with a network of transmitting stations so that shipping and observatories all over the world would be within range of at least two time signals per day. The development of this scheme was rapid, although it was temporarily delayed by the war. But now for many years the fundamental observatories all over the world send out their time signals for both general and astronomical purposes.

The importance of wireless time-signals for astronomy was immense: it made all the difference to navigation, it brought increased accuracy to precise surveying and to the astronomy of position, and it proved of enormous convenience to all observatories.

By observing the positions of the stars in relation to our horizon we can determine either our longitude or the time (Greenwich time) provided we know one of them; but even as we cannot both eat our cake and keep it, so we cannot find both longitude and time simultaneously. Before the invention of the ship's chronometer navigators used to determine the time from the position of the moon in relation to the stars; but owing to unexplained irregularities in the moon's motion the method was subject to large uncertainties. When Harrison invented the chronometer the difficulty was to a large extent overcome. But even chronometers could not be relied upon during a long voyage, and for each four seconds they were wrong the calculated position might be as much as a mile out. So soon as the time-signals were available sailors could always know their time well within the error of their sextant observations. Exactly the same advantages were offered to surveyors; for often they were far away from the telegraph line which was the only other means they had for checking the errors of their chronometers.

Those observatories that are concerned with the determination of fundamental star-positions—the determination of our celestial mile-stones—depend for the accuracy of their results on a correspondingly accurate knowledge of their longitude. For this purpose they used to get the necessary time-signals over the ordinary telegraph line. But there were always minute uncertainties, especially over long distances like the transatlantic cable, arising out of the speed at which the signal was transmitted. And so one of the first achievements of wireless in astronomy was the determination, between 1913 and 1914, of the difference in longitude between the observatories at Paris and Washington, D.C. This is now the standard method for determining the longitude of an observatory.

Apart from those that are occupied with the measurement of star positions, the majority of observatories require for their observations a knowledge of the time which is accurate to within a few tenths of a second. Before the war all these observatories had to devote a great deal of energy to the determination of their own time by means of the *transit instrument*. The satisfactory working of a transit instrument requires the expenditure of a considerable amount of highly skilled labour. Nowadays hundreds of transit instruments are lying idle, and instead astronomers correct their clocks from the wireless signals. Of course all these signals are ultimately derived from observations with transit instruments; but instead of numerous observatories wasting their time all doing exactly the same thing as one another, the service is carried on by just a few of them and made readily available for the rest through the medium of radio.

Stellar Evolution. In the year 1913 there was put forward a theory of stellar evolution which completely changed our ideas on the subject. Not only was the theory a brand new one; not only was it strongly backed by a wealth of observational data; but the theory itself had wide-reaching applications, and brought into a clearer light a number of allied problems which had previously been very imperfectly understood. As Professor A. Fowler remarked when in 1921 he presented the gold medal of the Royal Astronomical Society to its author, Professor H. N. Russell, the scope of the theory had become almost as wide as the universe itself. But although it was in 1913 that Professor Russell actually turned our views on stellar evolution inside out, we must look back quite a number of years to trace the origins from which his theory sprang.

The importance of the conception of the real or "absolute brightness" of the stars was first urged by Professor Hertzsprung, the great Danish astronomer. The stars we see on a clear night differ from one another in brightness considerably. But how much of the difference is real and how much is simply the result of their different distances? By absolute brightness we mean that brightness a star would have if placed at a certain standard distance from us. The standard actually adopted is one which light takes thirty-three years to traverse.[1] If we know how far away a star is we can always calculate how bright it would appear if placed at this standard distance of thirty-three light-years. Thus we can arrange all stars whose distances are known in order of their absolute brightnesses, and say how much brighter or fainter each one is than our own sun if placed at the same distance.

Now in 1905 when Professor Hertzsprung was investigating this matter there were only a very few stars whose distances were known; however, there were a large number for which the proper motions had been determined. As we saw before, it is reasonable to suppose that the nearer stars will, on the average, have larger proper motions than those that are more remote. This supposition had been borne out by observation which showed that, whereas there was practically no limit to the distances at which the stars might be, the velocities with which they were moving through the stellar system were confined within a comparatively small range. And so Professor Hertzsprung derived a statistical formula similar to that of Kapteyn which enabled him to calculate the most probable distances for the stars whose proper motions had been measured. And although the formula might give results that in individual cases were badly out, it ought to be reliable in the long run. In this way he got statistical results for the distances and the absolute brightnesses of many more stars than could be reached by the direct methods available at the time, and arrived at a discovery of tremendous importance. He found that while all the white stars were absolutely very bright the red stars fell into two groups, those which were absolutely very faint and those which were absolutely very bright; but there were no red stars of intermediate brightness. He traced, moreover, a rapid and perfectly regular diminution in brightness as one passed from the intensely bright white stars through the yellow and orange stars to the extremely faint red ones; but he found that there was an alternative branch to the sequence which contained yellow, orange and red stars of very great and about equal brilliance. In other words, there were two sequences among the stars: the " main sequence," containing the larger number, which fell off rapidly in brightness with increasing redness; the other sequence which preserved its great brilliance through the entire range of colours. The gap that separated the bright from the faint stars was widest among the red stars, narrower among the orange stars and barely distinguishable among the yellow stars. The white stars, all being bright, could be regarded indifferently as belonging to either or to both sequences. Professor Hertzsprung called all the very bright stars *giants*, and the faint orange and red ones he called *dwarfs*; and emphasizing the comparative rarity of the giants he picturesquely described them as " whales among the fishes."

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[1] The *scientific* astronomical unit of distance is the *parsec*—a portmanteau word, due to Turner, meaning the distance of a star whose parallax (or annual apparent shift) is one second of angle. 1 parsec = 3.26 light years. The distance at which absolute magnitudes are reckoned is 10 parsecs, or 32.6 light-years.

We saw how Secchi and Vogel had shown that the colours of the stars ran parallel with their spectral characteristics, that two stars of identical colour had closely identical spectra. Yet it seemed to Professor Hertzsprung difficult to believe that two stars of identical redness, one a giant and the other a dwarf and differing perhaps several thousand times in luminosity, did not show any fundamental difference in their spectra. Now a few years earlier Miss Antonia C. Maury, during her examination and classification of the Harvard photographs of stellar spectra taken for the Draper Memorial Catalogue, had made a curious discovery. There were certain groups of lines which in some stars appeared fuzzy and in other stars sharp; and in each spectral class there were stars, otherwise giving indistinguishable spectra, which could be separated into two groups according as they showed one or other of these two characteristics. It turned out that this was the very thing that Professor Hertzsprung was in search of. For he found that every star that showed the sharp-line characteristic of Miss Maury, the so-called c-characteristic, fell into his class of giant stars. Henceforward, therefore, it was possible to distinguish giant from dwarf stars by simply examining their spectra—there was no need to wait for their distances to be measured.

Now oddly enough these important discoveries of Professor Hertzsprung did not become at that time widely known; and so it was that in 1910 Professor Russell quite independently and from an entirely different angle arrived at very similar conclusions. As a graduate student at Princeton he came to England in 1902 and worked at Cambridge with Mr. Hinks on the photographic determination of the distances of the stars, measuring the annual shift in their positions due to the earth's orbital motion. In 1905 he went back to America and took with him the plates to measure them. By 1910 he was able to publish from them both the distances and the absolute brightnesses of fifty-five stars. And again there appeared the half-forgotten discovery of Professor Hertzsprung, the division of the stars into two groups: a main sequence growing rapidly fainter with increasing redness, and a subsidiary sequence consisting of stars of great brilliance irrespective of their colour. At this point Professor Russell had the brilliant idea that the two sorts of red stars, the extremely bright and the extremely faint ones, might represent two opposite stages in the life-history of the stars. Might not the two types be giants and dwarfs in the matter of size as well as luminosity? Might not the giants be stars in a very early stage of evolution, excessively rarefied and owing their great brilliance to their enormous volume and surface; while the dwarfs were shrunken with great age, very dense in comparison, and faint because of their consequently small volume and small radiating surface? His was in fact a reversion to the old and little favoured theory of Lockyer in which the stars were supposed to be born red and to die red; but now for the first time there was evidence to hand that there were in fact two types of red stars, the giants and the dwarfs.

And now we must pause to explain a few rather obvious points. The absolute brightness of a star—the brightness it would have if placed at the standard distance of thirty-three light-years—depends first on the luminosity of each little piece of its surface, and secondly, on the amount of its surface there is to shine, that is, upon the size of the star. Now the brightness of each little bit of the surface depends on the temperature: the hotter it is the more brightly it shines. What is more, the colour also depends on the temperature: like a poker placed in the fire, the colour of a star changes from red through orange and yellow to white as its temperature rises. And so from an examination of the colour and the spectrum of a star (for as we saw the two run roughly parallel) we can judge whether it is hotter or cooler than another star. If therefore a star is of a deep red colour and yet has a very great absolute brightness we can say at once that it must be enormously large; for since it is red it must be comparatively cool and each bit of its surface must have a low luminosity, hence its great total brightness can only be due to an enormously extensive surface. Thus, as Professor Russell maintained, the red giants must in comparison with the red dwarfs be stars of tremendous *bulk*.

Now a star can be large in one of two ways. It may be a star which is immensely rarefied and distended in which case it will be, despite its large size, of average weight. Or it may be a star of quite ordinary density in which case its weight as well as its size will be abnormally great. Adopting the first of these two alternatives a giant would differ from a dwarf in the same sense that a cricket ball differs from a football; their weights are much the same, but their sizes are very different. Adopting the second alternative they would differ in the same way that a large stone differs from a small stone; they are of the same sort of consistency and differ as much in weight as they do in bulk. Of course it was possible that a star was large or small for a mixture of these two reasons. It was, however, an essential part of Professor Russell's theory that the giants and dwarfs differed primarily in consistency and not in weight: that the giants were large because they were immensely rarefied and distended, and would in the course of evolution gradually shrink into small, dense dwarfs. On the other hand, if it were found that the difference in the brightness of giants and dwarfs was primarily due to a difference in their weights, then the bottom would be knocked completely out of Professor Russell's theory. One must remember that it was regarded as a fundamental law of nature that a body like a star isolated in space could never gain or lose one jot of its weight, unless it exploded and blew part of itself away. And so if giants were heavy and dwarfs were light stars they could not be regarded as representing different stages in an evolutionary sequence; instead they would have to be looked upon as belonging to two permanently distinct types of stars, types of stars distinguished by a pure accident of birth which no lapse of time could obliterate.

To decide between these two alternative possibilities it was clearly necessary for Professor Russell to collect observational evidence as to the weights and densities of the stars. It must be realized that of two stars, a giant and a dwarf, of similar colour, temperature and spectrum, the giant may have an absolute brightness several million times that of the dwarf. In which of these two respects, therefore, in weight or in density, did the giants and dwarfs differ sufficiently to account for such an enormous difference in brightness? The answer to this question would obviously be important in deciding between Professor Russell's evolutionary theory and the alternative possibility that giants and dwarfs represented types of bodies both initially and permanently distinct.

Now as regards stellar weights there were already to hand a considerable number of determinations; for, as we saw, it was possible to weigh any visual double star, of which the orbital motion had been measured, and any spectroscopic double star, which was also an eclipsing double star. Moreover for any spectroscopic double star that did not suffer periodic eclipse one could still get a minimum figure for the weight.

The available data at once showed that the variations in the weights of the stars were far too small by themselves to account for the enormous range in brightness. The majority of stars weighed between one-half and three times as much as the sun. No star was known with a weight less than one-sixth of the sun's weight; and it was extremely rare to find stars weighing more than ten or fifteen times as much as the sun. Thus the heaviest stars were little more than one hundred times heavier than the lightest; and this meant that if they were of the same density the heaviest stars would only have about twenty-five times the surface area, and therefore twenty-five times the brightness of the lightest stars. But a factor of twenty-five is a long way short of a factor of several million!

So Professor Russell next turned his attention to the question of stellar densities. We saw how the study of the light fluctuations of an eclipsing double star gave the relative diameters of the two stars in terms of the distances separating their centres; and how, if the star was also a spectroscopic double star, it was possible to convert the relative diameters of the components into actual miles and also calculate their weights. From the weight and diameter the density very easily follows: it is the weight divided by the volume, which is of course known as soon as the diameter is known. Unfortunately there were few stars for which it was possible to make all the necessary observations. Now it so happened that some years before Professor Russell had himself devised an ingenious method by which a close approximation to the density of eclipsing double stars could be got without any recourse to the spectroscopic observations. So he set about straightway to perfect the method and by the close of 1913 had, with the aid of Professor Shapley, measured the densities of no less than eighty-seven stars.

The results obtained were of the highest interest. In accordance with Professor Russell's prediction, the giants turned out to be extremely rarefied and diffuse stars, while the dwarfs were found to be compact, dense bodies. What was more, the difference in the consistency or compactness of the two classes of stars was sufficient to explain completely the difference in their brightness. We may take as an example two double-stars, one in the

Great Bear and one in the Southern Cross, which were among the first to be investigated. Both pairs of stars were similar to the sun in colour, spectrum and, therefore, in temperature; yet the first came out twice as dense as the sun, while the second was a million times more rarefied: one was denser than water, the other 550 times more rarefied than air. Assuming, for the reasons given above, that the weights of these stars are not very different from that of the sun, the more compact star would be rather smaller and less bright than the sun, while the more rarefied one would be enormously larger and about ten thousand times as brilliant. And this, it will be remembered, was just the sort of figure we were looking for. It unfortunately happens that very few of the spectroscopic and eclipsing double stars are placed far down towards the red end of the colour sequence. And this meant unfortunately that for just those stars—the reddest giants and dwarfs—among which the largest differences in brightness ought to exist, the observational data were not available. Thus for the present the theory could only be checked in its less sensational regions; and the exciting story of what observation was to achieve, subsequently, will have to be told in a later chapter.

Put briefly, the new theory stated that a star in its earliest youth was a tremendously distended and rarefied mass of gas glowing at a comparatively low red heat. As time passed the star contracted under its own weight and by falling in upon itself generated more and more heat. With rising temperature its colour passed gradually through orange and yellow to white, and at the same time each part of its surface grew more brilliant. But since the star was also getting smaller its total luminosity scarcely altered. By the time it had reached white heat it was so shrunken in size and so compact in consistency that the further contraction and *generation* of heat was markedly slowed down. From that point on therefore the star began to cool: it traced its steps back down the colour sequence, through orange and yellow to red, falling off the while steadily in brightness. The final stage was a star very small and very dense or compact; and, though it glowed with the same low red heat of its earliest youth, its total luminosity was now very small owing to its enormously reduced size. But one point should be particularly noted: on this theory the *weight* of the star was the same in its last as in its first stage: it had only contracted in size. Earlier than the red giant, or later than the red dwarf the theory did not specifically go. Presumably one must regard the diffuse red giant as crystallizing out of a still more diffuse, still cooler and therefore non-luminous and invisible nebula. And at the other end we can imagine the red dwarf gradually growing fainter and sinking into invisibility.

We saw that when astronomers weighed all the stars they could, they found them all very much about the same weight. True, some were heavier or lighter than the average, but a range in weight of about one to a hundred easily covered the majority of them. Such a variation was only to be expected in so stupendous an architectural scheme. But one thing required explanation: on the average the white stars were definitely heavier than the red stars, and the difference was more than could be ascribed to coincidence. The explanation of this was first given by Professor Russell and later worked out in greater detail by Sir Arthur Eddington. They found from mathematical and physical considerations that the point which a star reached in the colour sequence—the point of middle-age where it turned over from the stage of increasing to that of decreasing temperature—was dependent on its weight. Only a really heavy star could achieve white heat before turning out of the " giant sequence " into the " main sequence " and starting down the afternoon of its life. A less heavy star would only reach yellow heat; while a still lighter star would only become orange-hot or even remain red-hot and then start cooling. In fact a star with less than one-seventh of the sun's weight would never even become luminous—which explains why no star has ever been found with so small a weight. It follows from this that all the white stars must be heavy stars, while the red stars, which may be heavy or light, will have their average weight pulled down by their less heavy members.

The new theory of stellar evolution not only gave to astronomers a rational picture of the general outline of the life-history of a star, it had also a number of important by-products. The most interesting of these were the new lines of attack that it opened upon the problems of stellar distances. The theory had shown that all giant stars, whether red, orange, yellow or white in colour, were of approximately the same very great absolute magnitude; while on the other hand all dwarf stars, stars belonging to the " main sequence," had absolute magnitudes that fell off regularly as one passed down the sequence from white to red. In other words, if you knew that a star was a giant you could say within certain limits what its absolute brightness was; and if you knew that a star was a dwarf you could do the same thing provided you also knew what its colour was. And then of course if the absolute brightness of a star is known, that is the brightness it would have if placed at a standard distance, it is a simple calculation to find out how far away it is in order that it may appear to have the brightness which it in fact is seen to have.

Unfortunately this delightfully simple method cannot be applied to any individual star. In the first place a star must be fairly bright if its spectrum is to be photographed in sufficient detail to decide whether it is a giant or a dwarf. But even more important the rule given above is only statistically true: the absolute brightness of individual giant stars may vary considerably, while the absolute brightness of individual dwarf stars may vary still more.

But in the case of a star-cluster, where hundreds or even thousands of stars are so closely packed together in one small part of the sky as to make it certain beyond all coincidence that they form a physically related system and are all actually at the same distance from us, the method should give results of considerable reliability. If in such a cluster one can observe a minority of very bright stars, all approximately equal in brilliance but ranging from white to red, and a majority of fainter stars falling off regularly in brightness with increasing redness, then one can be sure that one has in that distant corner of the stellar system a family of giant and dwarf stars each one at the same distance from us. And so if one now applies to each star the new method of determining its distance the answer should theoretically come out in each case identical. For the reasons we have given this will not be so; but by taking the average of all the answers we ought to get a result that is not very far from the truth.

This most ingenious adaptation of Professor Russell's theory was due to Professor Shapley. The accuracy of the method was first checked by applying it to those extensive and comparatively nearby clusters whose distances had already been measured by Boss' method of convergent motions (see page 123). And when it was found that it gave good results it was used in the case of much more remote clusters to which Boss' method could not be applied. We saw how the method of Boss enabled astronomers to measure the distances of certain clusters ranging between about 50 and 150 light-years from us, but the new method extended their scope for measuring distances to clusters no less than 10,000 light-years away. Thus Professor Shapley had enormously lengthened the astronomical sounding-line. But one has to remember that the method was only a statistical one: it could be used for clusters where large numbers of stars were gathered together in one place, but it could not be used with any degree of certainty for individual stars.

In 1914 there was started an investigation by Professor Adams at Mount Wilson, California, and by Dr. Kohlschutter, who was on a visit there from Germany, which was destined to provide astronomers with the most powerful of all methods for measuring the distances of the stars. It had the great advantage over the method we have just described in being applicable to individual stars and not limited to clusters of stars. The principle of Professor Adams' and Dr. Kohlschutter's method was the *accurate* determination of the absolute brightness of a star simply from a study of its spectrum.

It will be remembered that the stars were found to differ remarkably little in their weights. No star was known which had less than one-seventh of the sun's weight, while stars weighing more than ten or twenty times as much as the sun were very scarce. So although they were built more or less to pattern a certain amount of licence was permitted. If all red giants were born of identical weight, then all giants from the red to the white stage would be of identical absolute brightness; similarly a dwarf of a given colour would have the same absolute brightness as all other dwarfs of that colour. But owing to this variation permitted at birth, some stars being born more massive than others, the above rule was only true on the average, while among individual giants or among individual dwarfs of similar colour there occurred quite considerable variations from the average absolute brightness. Many years before this we saw how Miss Maury at Harvard had noticed that stars of identical colour and with otherwise indistinguishable spectra might show a curious dissimilarity in some of their spectral lines: in one type these lines would be sharp, in the other fuzzy. Then a few years later Professor Hertzsprung had correlated these differences with his two types of stars, the giants and the dwarfs. In other words, the spectrum was more sensitive than the mere colour of a star; for it distinguished where the colours were indistinguishable. That which Professor Adams and Dr. Kohlschutter set themselves to discover was something in the spectrum even more sensitive than the Hertzsprung phenomenon, something which would distinguish not only giants from dwarfs but also greater from lesser giants and larger from smaller dwarfs.

We saw that in two stars of identical colour, one a giant and the other a dwarf, their spectra at first sight appear to be exactly alike. In each one the spectrum is seen to be crossed by hundreds upon thousands of dark lines; and if the two strips are placed side by side every dark line in one of them is found to correspond exactly in position with a dark line in the other. So far the only difference that had been noticed was that in the dwarf certain of these lines were fuzzy, whereas in the giant they were sharp. But Professor Adams and Dr. Kohlschutter soon discovered another sort of dissimilarity: there were certain lines that were stronger and darker in the giant and certain other lines that were stronger in the dwarf. When they came to investigate large numbers of stars they found that as they passed from the largest giants, through medium and smaller giants and then through larger and medium dwarfs to the smallest dwarfs, the first sort of spectral lines grew continuously weaker while the second sort grew continuously darker and stronger. Their next step was to select lines placed conveniently close together in pairs such that one line of the pair was of the type that was strongest in giants and the other of the type that was strongest in dwarfs. They were then able to measure on a photograph the relative strengths of the two lines in each pair and to draw curves correlating these measures with the absolute brightnesses of the stars determined by some independent method.

The original intention was, when a curve had been drawn up in this way from stars of which the absolute brightness was already known, to take stars of unknown absolute brightness, measure the intensity of their spectral lines and then get from the curve an *approximate* idea of their absolute brightness. Yet when the curve was finally produced it turned out to be far more valuable than anyone could ever have hoped. Instead of just telling one that a star was a large or medium giant, a small or a very small dwarf; it gave one an extraordinarily precise measure of its absolute brightness. The accuracy of this measurement was such that when one came to calculate from it how far away a star would have to be in order to look as bright as it did, the distance found was unlikely to be more than twenty per cent. in error.

It is impossible to lay too much stress on the importance of this achievement of Professor Adams and Dr. Kohlschutter. In the two other methods for measuring stellar distances there were serious limitations which do not apply to the new method. On the classical trigonometrical method in which the apparent annual to and fro shift of a star in the sky due to the earth's revolution round the sun was measured, the shift that had to be measured was smaller the further the star was away. In fact for stars more than one hundred light-years distant the shift was so small as to be practically unmeasurable. Again, in Boss' method, although it extends the range of distance measurement to about five hundred light-years, its applicability is strictly limited to stars belonging to a few extensive clusters that are near enough to us and moving sufficiently rapidly to show the convergent motion. But the new method of getting distances from the spectra, via the absolute brightness, was in no sense limited by a star's remoteness: one could make the determination with the same percentage error whether the star was near or far away; it was only necessary that the star should be bright enough for spectroscopic examination, and that after all was simply a question of having a sufficiently large telescope.

As a result of this discovery our knowledge of stellar distances advanced very rapidly. The method had the advantage of not requiring, like the other methods, laborious measurements at the telescope extending over months or years. One single photograph of the star's spectrum would often be sufficient; it could be studied at leisure; and what was more it could be made use of for many purposes apart from the determination of absolute brightness and distance. When in 1917 Professor Sampson, the Astronomer Royal for Scotland, presented to Professor Adams the Gold Medal of the Royal Astronomical Society, he remarked that it was no exaggeration to say that he had discovered a new door into the universe. With powerful modern telescopes stars of the same absolute brightness as the sun, that is to say, stars rather below the average in brightness, would still be bright enough for the necessary spectroscope measurements up to distances of about four hundred light-years. The average giant star which is about a hundred times brighter than the sun would be reached up to distances ten times as great, or about four thousand light-years; and finally, the occasional super giants, which might exceed the sun in brightness ten thousand times, would still be within reach of the new sounding-line at such immense distances as forty thousand light-years. There was only one other limitation to the method apart from apparent brightness: it could only be used in the case of the yellow, orange and red stars, the spectra of the white stars contained no lines that were suitable for the necessary measurements. However, about sixty per cent., or rather over half of the stars bright enough for the purpose, came within its reach, being yellow, orange or red in colour. Already several thousands of stars have had their distances measured in this way; and as larger telescopes are produced so the number of stars whose distances can be measured will increase.

Einstein's Theory. In 1915 there was published in Germany a theory which revolutionized our fundamental ideas on space, time and gravitation. This was Professor Einstein's Generalized Theory of Relativity. It is impracticable here to outline the story of the development of this theory: either the account would have to be disproportionately long, or it would be quite unintelligible to the non-technical reader. Moreover, relativity does not strictly belong to the domain of astronomy; no more, in fact, than does Euclidean geometry or Newtonian mechanics. Like them it belongs to the domain of mathematical physics. We shall therefore deal only with those aspects of relativity which are especially connected with astronomy, with, what are known, as its "astronomical consequences."

Einstein's achievement was to replace the two separate sciences of geometry and mechanics by the single science of relativity. Relativity is in fact a multi-dimensional geometry which supersedes the three-dimensional Euclidean geometry; while the law of the new geometry is Einstein's law of gravitation.

But though the underlying ideas of relativity are fundamentally different from the ideas of the classical theories it has replaced, the results it arrives at are, in almost all cases, indistinguishable from the results arrived at by the classical theories. There are at present only a few cases in which the results predicted by the old and the new theories differ by an amount that is large enough to be detected by the most accurate measurements. And it so happens that the three most important are provided by astronomy.

According to both theories a planet revolves round the sun in an elliptical orbit; but whereas classical theory maintains that the ellipse is fixed in space, relativity predicts that it should slowly revolve—just as an elliptical bacon-dish may be pictured as slowly revolving on the dining-room table. The rate at which the ellipse turns round is excessively slow, and will only be detectable if the planet is revolving very rapidly round the sun and if its orbit has a considerable degree of ellipticity. The effect should, in fact, only be appreciable in the case of Mercury, the planet which revolves round the sun most rapidly and in the most elliptical orbit. Now it had long been known that Mercury's orbit was subject to a change of this sort, and in 1859 Leverrier had tried to explain it as the result of the gravitational disturbance exerted by a hypothetical planet revolving within the orbit of Mercury. But this, and other explanations put forward subsequently, had never been completely satisfactory; and Mercury had come to be regarded as the one planet for which the Newtonian law was inadequate. Einstein's theory not only explained why the orbit of Mercury was revolving, but also gave for its revolution a rate practically identical with that found from the best observations.

Again Einstein's theory required that a ray of light, like a material particle, should be deflected from its normal course in a straight line when it passed near a heavy body like the sun. According to Newtonian theory a ray of light might or might not be deflected from the straight path when passing near a heavy body, but if it was so deflected then the amount of the deflection should only be half that required by Einstein's theory. Einstein pointed out that this prediction could be tested during a total eclipse of the sun. At such times the sun is completely blotted out by the moon, and it is possible to photograph stars close up around the sun's edge. The light from such stars must obviously pass very close to the sun's surface on its way to our cameras; it should therefore be deflected from the straight line, and cause the stars to appear slightly shifted from the positions they occupy at other times of the year when the sun is no longer in their midst. In 1916 the late Professor de Sitter of Leiden brought to the notice of British astronomers the great importance of Einstein's theory and pointed out its astronomical consequences. And in the following year Sir Frank Dyson, then Astronomer Royal, proposed that every effort should be made to test the theory at the eclipse of 1919, as the sun would then be almost ideally situated in the midst of an unusually large number of comparatively bright stars. As a result of the untiring efforts of Sir Frank Dyson the necessary preparations were made, so when the war came to an end it was possible to send out two well-equipped expeditions. Dr. Crommelin and Mr. C. R. Davidson went to Brazil, and Sir Arthur Eddington and Mr. Cottingham went to the Isle of Principe off the west coast of Africa. The results of both expeditions provided striking confirmation of Einstein's prediction. Exactly similar results were obtained in 1922 by an American expedition to Australia and at more recent eclipses by various other expeditions.

Einstein's third prediction was that near the surface of a massive body like the sun all physical processes should take place more slowly than they do at the surface of the much less massive earth. In particular the atomic vibrations which gave rise to spectral lines should be slightly slower for the atoms in the sun's atmosphere than for the corresponding atoms in a terrestrial laboratory. This would result in the lines in the solar spectrum being displaced slightly towards the red end of the spectrum as compared with the same spectral lines photographed in the laboratory. Unfortunately there are several other effects which produce a similar type to shift in the spectral lines; and it was not until after many years of careful study that Mr. Evershed in India and St. John at Mount

Wilson were able to conclude independently that after making full allowance for all other effects there remained a shift in the lines equal to that predicted by Einstein. As we shall see later, however, a much more decisive confirmation of the " Einstein shift " was obtained in 1925 from the study of the faint, but abnormally heavy, star which revolves round the bright star Sirius.

The successful confirmation of the theory by means of these three crucial tests was in itself a very powerful argument in favour of the truth of the theory; but when in addition one took into account the essential simplicity of the theory, its intrinsic logical consistency and its freedom from *ad hoc* hypotheses the argument in favour of it became overwhelmingly strong. It cannot now be doubted that the Generalized Theory of Relativity has passed into the realm of established science.

The next step was to see whether the theory of relativity offered any escape from that difficult conception of an *infinite* universe which had so long been the bugbear of physicists and philosophers. In 1917 Einstein showed that by extending his theory it was possible to see how at great distances space *might* curve round and close up on itself; and thus be finite yet unbounded, in the same sort of way as is the curved surface of the earth. In that same year de Sitter showed that there was an alternative method, different from Einstein's, for extending the theory so as to close up space; and some years later the Abbé Lemaître discovered yet another method. Now it so happens that the extensions of the theory of relativity due to de Sitter and to the Abbé Lemaître carry with them the requirement that at very great distances celestial objects should be receding from the earth with velocities which are proportional to their distances from us. We shall see later that it has been found that the spiral nebulae, which are the most distant bodies we can see, are with very few exceptions receding from us with prodigious velocities which are proportional to their distances from us. But whether this phenomenon is to be explained by one or other of these two theories, or by the quite different theory that has recently been put forward by Professor Milne of Oxford, is not yet decided. For it must be realized that the various extensions of the theory of relativity have in no sense the same degree of certainty as the Generalized Theory of Relativity itself.

CHAPTER IX

SOUNDING THE DEPTHS OF THE UNIVERSE

As so much of what we shall have to discuss in subsequent chapters has been accomplished with large telescopes, it will be well to begin by saying something about the two largest telescopes in the world which were put up in 1918. Both of these instruments were reflectors. The first to be erected was a telescope with a mirror six feet in diameter at the Dominion Observatory, Victoria, British Columbia. The other which followed a few months later was a 100-inch reflector erected at the Mount Wilson Observatory in California.

Towards the end of last century the refractor had reached more or less to the theoretical limit beyond which its diameter could not increase. These approximate limits were achieved in the 36-inch of the Lick Observatory and in the 40-inch of the Yerkes Observatory. About the same time the new silver-on-glass reflector had made its appearance. By 1900 the largest reflector of this type was the 5-foot made by Common. But this instrument had not been entirely successful; and there is no doubt that the finest instrument of this kind at the time was the three-foot mirror made by Calver, mounted by Common, and subsequently erected after being re-figured by Grubb as " The Crossley Reflector " at the Lick Observatory.

The next advance in the construction of large reflectors was due to Professor Ritchey, who is probably the greatest living authority on the making of large mirrors and on the mounting of them. In 1902 he finished at the Yerkes Observatory a magnificent mirror 60 inches in diameter. When in 1904 a new observatory was established as an off-shoot of the Yerkes Observatory on the summit of Mount Wilson, Professor Ritchey was put in charge of the new mechanical and optical workshops. At first the trail up the mountain was not wide enough for the 60-inch telescope to be taken up; but shortly afterwards this was done and the then largest working telescope in the world was successfully erected at an altitude of nearly 6,000 feet. The new instrument represented a tremendous advance on any reflector that had preceded it, and from the point of view of sheer light-grasp it far outstripped any refractor. Even the Rosse telescope with its 72-inch metal mirror was its inferior in light-gathering power.

But the 60-inch was not only a monster telescope, it was lavishly equipped with all the elaborate accessories that are necessary for spectroscopic and photographic work.

It must not be supposed that the prime object in making a telescope is that it should be the largest in the world; and that when another larger one is built the first is only capable of doing work of secondary importance. It is of course essential for certain purposes to make telescopes as large as possible, though for other purposes smaller instruments are better; but it is very much more important to make *large numbers* of large telescopes. The bigger the telescope is the further it penetrates into space, the more numerous are the objects it reveals for study and the greater are the number of problems it raises for elucidation. The simplest of these problems may require for their solution the making of hundreds or even thousands of observations. And if one telescope however large was to be used alone to unravel the mysteries of astronomy the situation would be just as hopeless as the one which the Walrus contemplated as he strolled along the shore:

" If seven maids with seven mops
" Swept it for half a year,
" Do you suppose," the Walrus said,
" That they could get it clear?"

And so although the 60-inch at Mount Wilson was not surpassed until 1918, a number of other large reflectors, between thirty and forty inches in diameter, sprang up in the meantime in different parts of the world.

We have said that the optical perfection required in the glass disc for a mirror need only be skin-deep. But this is only true in comparison with the extreme degree of perfection necessary in the glass discs for a lens. Although it is not difficult to obtain glass of the required quality for mirrors of moderate dimensions, it was a very different matter getting the glass for the 72-inch and the 100-inch mirrors. A 100-inch disc was finally produced by the French Plate Glass Co. of St. Gobain, France, after they had made many preliminary trials with smaller discs. The disc was shipped to Mount Wilson and was tested there, but it was rejected on the grounds that it was not sufficiently perfect. But though the St. Gobain company made numerous attempts to produce a better disc, the only ones which they successfully cast cracked during the process of annealing. Then came the unfortunate destruction during the war of the factory of this famous glass company; so it was decided after all to use the disc which had originally been rejected. The grinding, figuring and polishing of it were entrusted to Professor Ritchey. And the mirror, which weighed over four tons, was found when finished to possess after all a high degree of perfection.

The disc for the 72-inch was also made by the St. Gobain company and was shipped to America only a week before the outbreak of war. For testing this mirror during the figuring process a second mirror 55 inches in diameter had been ordered from the same company. Unfortunately the war prevented its delivery, and a whole year was wasted in fruitless attempts to obtain a similar disc from American glass makers. In the end it was necessary to adopt a more complicated method of testing, and to make use of a 33-inch mirror which happened to be ready to hand. The grinding, figuring and polishing of the 72-inch mirror was done by the Brashear Company of Pittsburgh, Pennsylvania.

One sees from this how completely dependent astronomers were on a single firm when it came to the production of glass on a really large scale. Glass-making was still much more of an art than a science; and the greatest artists in glass-making, who alone could produce the biggest discs, were extremely few and far between. It is in fact only in the last ten years that the Americans have succeeded in reducing glass-making to a scientific basis by sorting out the essential factors upon which its success depends. About 1928 the Bureau of Standards in Washington produced a 70-inch disc for the Perkins Observatory in Ohio; and then more recently the Corning Glass Co. of New England produced first the disc for the 74-inch reflector just mounted at the David Dunlap Observatory at Toronto, and then two discs each 200 inches in diameter for the new telescope now under construction for Mount Palomar.

The mounting of the 100-inch and the 72-inch telescopes were engineering feats of no mean order. The moving parts of the 100-inch weigh about 100 tons and those of the 72-inch about 45 tons. There must be no sag in the tubes or the mountings; the telescopes must be capable of moving with perfect smoothness and accuracy in following the stars across the sky; and, further, it must be possible to control these movements delicately and rapidly, and to turn the telescope quickly from one part of the sky to another. The 100-inch telescope is so rigid that a man can clamber out along the tube to its upper end and yet barely displace a star in the field of view. The metal dome covering this telescope is 100 feet in diameter. In both instruments the movements of the telescopes themselves, the rotation of the domes and the movements of the shutters and of the observing platforms are all controlled by electric motors. It may help one to realize the gigantic labour involved in the construction of a telescope of this size when one is told the amount of money spent upon it. The 100-inch cost 540,000 dollars, in spite of the fact that much of the work was done by optical and mechanical experts on the staff of the observatory.

Until May 1935 the 100-inch and the 72-inch were the two largest telescopes in existence. The second place was then taken by the 74-inch which Sir Howard Grubb Parsons and Co., Ltd. built for Toronto. Moreover within a few years the 100-inch itself is likely to be superseded by a 200-inch. Already, as we have said, the glass disc for this giant mirror has been made; and the plans for its grinding and figuring and subsequent mounting are now being pushed ahead. Though it will be the chief instrument of the Mount Wilson Observatory it will actually be mounted on Mount Palomar some ninety miles from Mount Wilson.

During the last thirty years big reflectors, equalling or exceeding in size the largest refractors, have been erected in different parts of the world. 36-inch reflectors are in use at Greenwich, in Edinburgh, in Santiago, Chili and in Tucson, Arizona. There is a 38-inch at Ann Arbor, Michigan. There are 40-inch reflectors at Meudon, Paris, at Bergedorf near Hamburg, at Stockholm, at Geneva, at Como in Italy, at Simeis in South Russia, at Flagstaff in Arizona and at Washington, D.C. And finally the Harvard Observatory has recently sent to its stations at Bloemfontein in South Africa, and at Oak Ridge, Mass. the two 60-inch mirrors made by Common, the imperfections of which have been removed by the process of refiguring.

One notices at once that all these large reflectors with two exceptions are situated in the northern hemisphere. Now this deficiency is a most serious matter; and the attempt which is now being made to rectify it undoubtedly constitutes one of the most significant developments of recent years. From the northern hemisphere there is a considerable portion of the sky round the south pole of the heavens which is completely out of reach. The observatories of the southern hemisphere—in South Africa, Australia and South America—are many of them well equipped with comparatively small instruments capable of dealing with the astronomical problems of twenty or thirty years ago. In studying such problems we have, therefore, got data well distributed over the whole sky. But for many of the more recent problems of astronomy, which can only be investigated with the largest instruments, the data are sadly lop-sided. A similar situation had arisen on previous occasions in regard to particular problems. In 1820 the Admiralty had founded the observatory at the Cape of Good Hope to extend the Greenwich work on the fundamental positions of stars to the southern hemisphere. In 1833 John Herschel went to South Africa to complete his father's survey of double stars and nebulae. In 1891 the Harvard Observatory set up a station at Arequipa in Peru, in order to continue their survey of stellar spectra and their studies of star clusters and variable stars in the southern skies. In 1927 the Harvard station at Arequipa moved to Bloemfontein because of the better conditions existing there. About the same time the Ann Arbor Observatory, Michigan, also set up in Bloemfontein a 27-inch refractor for the study of visual double stars. Finally, two other large refractors were erected in Johannesburg: a 26½-inch at the Union Observatory for the investigation of visual double stars, and a 24-inch photographic refractor at the station belonging to the Yale Observatory, for the measurement of stellar distances and stellar motions. But it was not until 1933, when Harvard refigured the 60-inch Common reflector and set it up at their Bloemfontein station, that there was at last one instrument in the southern hemisphere capable of the sort of work which was being done with the great reflectors in the northern hemisphere.

It is good to know that in a few years' time another still larger reflector will be at work in South Africa. This is a 74-inch reflector which is now on order from Sir Howard Grubb Parsons and Co., Ltd., for the Radcliffe Observatory. The Radcliffe Observatory was founded at Oxford in 1771 and antedates by a hundred years the Oxford University Observatory. For some years the observatory had been occupied with the photographic determination of the distances and motions of the stars with a 24-inch photographic refractor. The original programme of work was nearing completion when the trustees were asked if they would sell the site of the observatory to Sir William Morris (now Lord Nuffield) for presentation to the Radcliffe Infirmary which was urgently in need of room for expansion. It seemed to be an excellent opportunity for transferring the observatory to a more suitable site and at the same time for erecting a large reflector in the southern hemisphere. The sale was completed for £100,000 and at the end of 1929 Dr. W. H. Steavenson went out to Pretoria with a 6-inch refractor to select the most favourable locality for the site of the intended observatory. But just at the time when the site was in process of being selected an unfortunate hitch arose. It was found that it would be necessary to obtain the permission of the High Court before the funds of the Radcliffe Observatory could be transferred out of the country, and that there would be many objections raised against the proposed transference. It was not until 1934 that the necessary preliminaries had been completed and the matter was able to be brought before the High Court. Although at one time the outlook had appeared very black, the case when it came before the court went through surprisingly smoothly and quickly and the desired permission was granted. The 74-inch telescope which has been ordered will be similar to that recently made by the same firm for the David Dunlap Observatory at Toronto. The importance of this telescope to astronomy will be immense, for the amount of work which has to be done to fill in the gap in our knowledge of the southern skies is far too much for the Harvard reflector single handed. Moreover, it is good to know that the country which in the eighteenth century laid the foundations of sidereal astronomy will again be able to take a leading part in the observational development of this subject.

A recent instrumental advance of the greatest importance has been the invention of the method of coating telescope mirrors with aluminium instead of silver. The aluminized mirror has several noteworthy advantages. It lasts for years, if not permanently, when properly applied; whereas the silvered mirror rapidly tarnishes and requires resilvering every few months. A freshly silvered mirror has a reflecting power slightly superior to that of an aluminized mirror, as far as the visible region of the spectrum is concerned. But after some days the highest polish wears off, and thereafter the silvered mirror becomes inferior and increasingly inferior to the aluminized mirror. The chief advantage of the aluminized mirror is its markedly superior reflecting power in the violet and particularly in the ultra-violet regions of the spectrum. Consequently in photography, for which the largest reflectors are almost exclusively used, the exposure-time required with an aluminized mirror is considerably less than that required with a silvered mirror. But the greatest advantage is obtained when one comes to the photography of stellar spectra, for the aluminized mirror extends the spectra into the ultra-violet appreciably beyond the furthest limit reached by the silvered mirror.

The method of aluminizing mirrors was mainly developed by Dr. John Strong of the Californian Institute of Technology in 1932 and 1933; and in the latter year he succeeded in aluminizing the 36-inch mirror of the Crossley reflector. It was not until

1935 that the method was able to cope with the great mirror of the 100-inch telescope.

The process of aluminizing is complicated; and since the aluminium has to be deposited by evaporation in a fairly high-grade vacuum, elaborate apparatus is required. As yet it is only in America that the largest mirrors can be aluminized; and elsewhere the aluminization of even small mirrors is by no means invariably successful. Undoubtedly in a few years time, when the correct technique has been more widely learned, the aluminium-on-glass reflector will be universally employed.

The spiral nebulae are perhaps the most remarkable objects in the heavens. We have already referred to Lord Rosse's discovery with his 6-foot reflector of their spiral structure; and we have mentioned the suggestion which had been put forward from time to time that they were systems of stars external to and independent of our own. The elder Herschel had first suggested this idea of " island universes." For it had seemed to him that the resolution of a nebula into a swarm of stars was merely a question of telescopic power; and he had concluded that some of these distant stellar systems were comparable in size with our own. But later he was forced to abandon this theory owing to his growing conviction that in a great many of the nebulae the luminous haze was definitely "not of a starry nature." There was, however, another phenomenon which seemed to be opposed to the view that the nebulae were systems independent of our own. For Herschel had noticed a strong tendency among them to congregate towards the two regions of the sky most distant from the Milky Way. That the nebulae should so deliberately avoid the plain of the stellar system was itself an argument against their being independent of that system. The view that all nebulae were in reality distant systems of stars was again revived when the observers with the great Rosse reflector thought they could detect in a large number of these bodies a definitely stellar structure. The next stage was reached when Huggins showed that whereas some nebulae gave a gaseous spectrum, others gave the spectrum of ordinary starlight. So it now seemed that nebulae were of two kinds: those that were really nebulae, and those that were comprised of discrete particles, possibly of stars, seen at a great distance. And since the spiral nebulae were shown by their spectra to belong invariably to the latter class, the theory that they were island universes again found favour in the eyes of a few astronomers. Nevertheless there were two arguments against this theory which seemed to be irrefutable, The first was the tendency, mentioned above, for the nebulae to be concentrated towards the poles of the Milky Way. Although this tendency had been recognized by Herschel, it was R. A. Proctor in 1869 who first showed by accurate charting how very striking it was. The second argument was put forward by Proctor some fifteen years later. On one occasion a " new star " had blazed up in a spiral nebula and reached approximately naked-eye visibility. If it was assumed that these nebulae were stellar systems so remote that even the largest telescopes could not reveal their individual stars, then the appearance within them of a single star bright enough to be visible to the naked eye would imply an outburst so terrific as to seem quite beyond the limits of possibility. But although during the close of last century and during the first two decades of the present century the theory of island universes was generally regarded with strong disfavour, several eminent authorities rose up from time to time to champion it. And it is interesting that one of the strongest supporters of the theory in the days before the war was Sir Arthur Eddington, who, as we shall see later, has done so much in the last twenty years towards placing it on its present firm foundations.

About 1914 Dr. V. M. Slipher of the Lowell Observatory started to photograph, on a scale larger than had been attempted before, the extremely faint spectra of the spiral nebulae. His object was to measure the positions of their spectral lines in order to find out with what velocities they were moving towards or away from the earth. By 1917 he had successfully studied fifteen spirals and found that all of them were moving with velocities that were extremely high when compared with those of ordinary stars. Moreover with very few exceptions their movements were away from the earth. About the same time Dr. van Maanen at Mount Wilson published the results of some careful comparisons he had made of photographs of spiral nebulae taken several years apart. On photographs one sees the arms of these spirals broken up into condensations; and Dr. van Maanen, by comparing the photographs was able to detect and measure the motions of the condensations outwards along the arms in seven separate nebulae. The results were remarkable and seemed to deal the death blow to the island universe theory. For if the nebulae were really as far away as that theory required the very considerable changes found on the photographs would indicate that the velocity with which the condensations were travelling outwards along the spiral arms was impossibly large. And so it appeared that after a long and rather vague existence the island universe theory had at last died a natural death.

In 1918 another class of celestial object came very much to the fore. These were the " globular star clusters " the precise relation of which to our stellar system had long been a puzzle to astronomers. In this year, however, Professor Shapley, then at Mount Wilson, published the results of an extensive research into their distances which helped very considerably towards the elucidation of the problem.

Star-clusters are of two kinds: the " open clusters " and the " globular clusters." They are quite distinct both in their appearance and in their distribution in the sky. As their name implies the open clusters consist of loosely packed stars often arranged very irregularly. Some of these clusters are visible to the naked eye, and in a few, such as the Pleiades, one can see individually some of their brighter stars without optical assistance. In many, what looks like a patch of luminous haze to the unaided eye is resolved into stars with a field glass or a small telescope. The separate stars in such clusters usually number a few hundred. Finally these clusters show a very marked tendency to congregate towards the plane of the Milky Way. In Chapter VII we referred to what are called " moving clusters." It will be remembered that whereas some of these *looked* like clusters and were in fact included among the " open clusters," there were others so widely scattered over the sky that their claim to be regarded as clusters was only to be inferred from the fact that the individual stars were all moving together through space. The closeness with which the stars in these clusters appear to be packed must largely depend on their distance from us. The further away a cluster is the smaller will be the area of the sky over which its individual members extend. If a cluster is close to us, or if we are actually situated within it, it will clearly cover a large area of the sky or even extend all over the sky, and its appearance as a cluster in the heavens will consequently be indefinite. Thus we see that the "open clusters" include all objects, from those that are so widely scattered that their nature as a cluster is only to be inferred from a study of the motions of their stars, to the densely packed star clusters of the Milky Way.

The globular clusters are quite different. The three brightest are just visible to the naked eye, though barely to be distinguished from an ordinary faint star. Looked at with a large telescope they are seen to be perfectly round objects consisting of myriads of faint stars literally crammed together, especially at the centre, where the individual stars are scarcely to be separated. They are all extraordinarily similar to one another as to their internal arrangement. They differ from one another mainly in their apparent size; and this strongly suggests that they are all of similar dimensions, while those that appear smaller do so simply because they are further away. The stars in these clusters are much more numerous than the stars in the open clusters, and long exposure photographs of one of the larger clusters will show at least fifty thousand separate stars within it. But one of the most striking differences between the globular and the open clusters is their distribution in the sky. As we saw, the open clusters are most numerous within the belt of the Milky Way and fall off in numbers very rapidly on either side of it. The globular clusters on the other hand are entirely missing from the belt of the Milky Way, are most numerous at a certain distance on either side of it, and are still quite numerous in directions at right angles to the Milky Way. But there is still a more puzzling matter. The Milky Way forms a belt which runs completely round the sky. But the globular clusters, lying as they do on either side of the Milky Way, above and below it, have that relation to only one half of the circumference of the Milky Way. Round half the circuit of the Milky Way we find large numbers of globular clusters situated above and below it; but round the other half of the circuit of the Milky Way we find practically no globular clusters. There are just a few, it is true: but whereas there are eighty-two in one hemisphere of the sky there are only four in the other.

This peculiar distribution of the globular clusters is best explained by the well-known model of the " buttered bun." Imagine a currant bun which is, though very nearly round, slightly flattened or spheroidal in shape. Let it be cut in two and spread in the usual way with a thick layer of butter. The layer of butter which has the form of a flat circular disc will represent the system of the stars, the gaseous nebulae and the open star-clusters. The currants will represent the various globular clusters scattered at random throughout the bun but avoiding, as a result of the cutting and spreading, the actual layer of butter. If the earth with the solar system were situated in the middle of the bun, exactly at the centre of the buttery disc, we should obviously see the stars, open clusters and gaseous nebulae concentrated in the directions of the circumference of the disc, in a belt circling the sky. But the currants, or the globular clusters, would be seen indiscriminately in all directions in the sky except in the directions of the circumference of the buttered disc. But this is not yet quite what we require. Instead we must assume that the earth, although situated within the disc of butter, is not at its centre but some considerable distance from its centre, say about half-way towards the edge or rim of the bun. And now when we look out from this new situation, although we still find ourselves surrounded on all sides by the girdle of butter (or the Milky Way), we shall find that the currants (or globular clusters) are much more numerous in one half of the sky than in the other.

Although the model of the buttered bun provides a reasonable representation of the system of the globular clusters, it obviously could only be regarded as provisional so long as we knew nothing about the distances of these objects. It was quite conceivable, for example, that the system of the stars was very small in comparison with the system of the globular clusters, and that the portion of the bun which was buttered was but a small fraction of its cut surface.

The principle underlying the investigation of Professor Shapley into the distances of the globular clusters takes us back to a most important discovery in regard to the so-called Cepheid variable stars. Superficially the Cepheid variable stars wax and wane in brightness in a manner very similar to the eclipsing variable stars discussed in a previous chapter. Their light variation is of clock-like regularity, and the changing positions of their spectral lines suggests that they alternately approach and recede from us just like the brighter component of an eclipsing variable star. But when these variations in their brightness and their spectra are examined in detail it is found that they do not fit in with the explanation that they are due to two stars revolving round one another and periodically eclipsing one another. On the one hand they brighten up more rapidly than they fade, whereas in an eclipsing variable star the two processes take exactly the same time. Then again their spectra show them to be receding from us at their maximum speed just at that phase of their light variation when, if they were eclipsing variable stars, they ought to be stationary. The explanation now generally accepted is that a Cepheid variable star is a single star which is in a state of pulsation, its surface alternately expanding and contracting. This theory was put forward about 1915 by Professor Plummer, when he was Astronomer Royal of Ireland, and is now generally regarded as giving the best explanation of the observed changes in the brightness and the spectra of these stars.

But at the moment we are not concerned with the why and wherefore of Cepheid variation. The discovery in which we are now interested was of quite a different kind. In the southern hemisphere not far from the south pole of the heavens there are two great clouds of stars known as the Magellanic Clouds which are rather like portions of the Milky Way that have been broken off from it. In 1912 from a study of a long series of photographs of the smaller of these two clouds, Miss Leavitt of the Harvard Observatory discovered within the cloud twenty-five faint variable stars the variation of which was typical of Cepheids. But the remarkable thing was that when Miss Leavitt had determined the periods in which these twenty-five stars varied in brightness, she found that the lengths of their periods ran almost exactly parallel with their apparent brightness. The brighter the star was the more slowly it waxed and waned. The full significance of this curious relation was immediately realized by Professor Hertzsprung of Leiden, the discoverer of giant and dwarf stars. He saw at once that the fact that Miss Leavitt's twenty-five Cepheid variable stars were all in the smaller Magellanic Cloud, meant that they could all be regarded as being at approximately the same distance from the Earth. If therefore there was this relation between their periods and their *apparent* brightness, there must exist the same relation between their periods and their *absolute* brightness or candle-power. For clearly if there are two stars at the same distance from us and one of them appears twice as bright as the other, then it follows that the brighter star must *really* be twice as luminous as the fainter one. Consequently if one could only find the candle-power or absolute brightness of a single Cepheid in terms of the candle-power or absolute brightness of the sun, one could immediately say what was the candle-power of any other Cepheid from a determination of its period of variation. And directly one knows this, how bright the star really is, it is a straightforward sum in division to discover how far away it must be in order to appear as bright as it looks. So Professor Hertzsprung turned his attention to the Cepheid variables situated in the nearer parts of the stellar system in the hope of finding some close enough for their distances, and hence their candle-powers, to be determined by one of the established methods. As none of them was near enough for its distance to be measured by the ordinary direct method, he had to adopt the statistical method discussed in Chapter VII, in which the distance of a group of stars is obtained from a consideration of their apparent motions in the sky. He was able to collect suitable data for thirteen stars and determined for each of them their distance and candle-power.

In this way Professor Hertzsprung determined the scale, or " zero-point " as it is called, of the relation between period and candle-power; so henceforth when he knew the period of a Cepheid he could at once deduce its absolute brightness. By applying his relation to the twenty-five Cepheids in the Small Magellanic Cloud he obtained for them and for the cloud a distance of 30,000 light-years. Thus Professor Hertzsprung was the first man to realize that the Cepheids were continually flashing out messages of fundamental importance to the astronomer, and to arrive at a method for decoding those messages.

Professor Hertzsprung's relation between the candle-power and the period of light variation of the Cepheids is known as the " period-luminosity law." But the key to the law—its scale or zero-point—which had been derived from the study of only thirteen stars, was obviously to be regarded as provisional. Between 1916 and 1918 Professor Shapley investigated the distances of a much larger number of the nearer Cepheids, and thus obtained a much more reliable estimate of the all-important zero-point. Adopting this new value the distance of the Small Magellanic Cloud was increased to about 100,000 light-years. Still more recent investigations have resulted in the value of the zero-point dropping back again slightly, giving a distance for the cloud of about 85,000 light-years.

It will be seen from the above that Professor Hertzsprung's discovery was of the most far-reaching importance. Wherever a Cepheid could be identified, by its characteristic type of light variation, its distance could at once be determined: one simply had to measure its apparent brightness and observe the period of its light variation. And further, whenever one found a number of Cepheids involved together in a star cluster and all at the same distance from us, one could be quite certain that they really formed part of the cluster, and so deduce the distance of the cluster itself.

It was this particular application of the new principle which Dr. Shapley put into practice at Mount Wilson between 1916 and 1918. In a number of the globular clusters typical Cepheids of short period were found. To these the " period luminosity law " could be applied directly and their distances immediately found. The problems next arose as to how the method could be extended to the smaller and presumably more distant clusters in which no Cepheids were visible. Dr. Shapley found that in every cluster which contained Cepheids these stars all had periods around half a day which gave them a candle-power about 100 times that of the sun. He also found that, with great regularity, the average brightness of the twenty or thirty brightest non-variable stars in each of these clusters was about ten times that of the average brightness of the Cepheids in the same cluster. On the assumption that this relation held good for all the clusters, he was thus able to extend his distance measurements to those which were too remote for their Cepheids to be visible. His next step was to measure the diameters on the photographic plates of all those clusters of which he had determined the distances by one or both of these two methods. The apparent diameters came out very closely in inverse proportion to the distances, which meant that the real diameters of the clusters were all approximately the same, and that the ones which appeared smaller did so simply because they were further off. This last result was as a matter of fact not surprising. The extraordinary similarity in the appearance of the globular clusters had already suggested that they were systems built very much to a set pattern and possibly also to an approximately fixed scale. Nevertheless its confirmation in this way was of great importance. It gave a very valuable check upon the two methods of determining the distances of these clusters; because one could scarcely suppose that if those methods were really giving erroneous results they should conspire to make the diameters come out so nicely the same. Furthermore it was now possible, in the case of the few clusters so far away that the individual stars could not be distinguished, to estimate their distances merely by measuring their apparent diameters. One cannot help admiring the dramatic rapidity with which in this short time Dr. Shapley pushed ahead into the remoter parts of the universe, and the ingenuity with which he added first one and then another extension to his cosmic sounding-line and finally touched bottom at the furthest of the globular clusters.

There were nearly ninety globular clusters known in the sky and in 1918 Dr. Shapley published the distances of all of them. The nearest came out at a distance of 20,000 light-years, the majority were three or four times that distance, and the most remote of them all was about 200,000 light-years. The main source of uncertainty in these figures is the one mentioned just now. The relation between the period and the luminosity could be regarded as reliable, but there was still some uncertainty as to its so-called " zero-point."

But how did the measured distances of the globular clusters fit in with the model of the buttered bun? From his 1918 measures Dr. Shapley was straightway able to construct a model of the system of globular clusters and to place each individual currant in its proper position in the bun. As defined by the position of the currants within the bun the diameter of the bun turned out to be about 260,000 light-years. The earth with the solar system was found to be situated a long way from the centre of the bun, in fact about half-way from its centre to its edge. At this time the accepted view as to the diameter of the stellar system itself was that derived by Kapteyn who put it, as we saw, at 55,000 light years, which is about one-fifth of Dr. Shapley's diameter for the system of globular clusters. By combining these two separate sets of results together in our model we see that the disc of butter representing the stellar system can only be spread over about one-fifth of the cut surfaces of the bun and must lie about half-way from the centre to the edge of the sandwich.

Dr. Shapley, however, took a different view of the situation. He felt that the statistical methods of Kapteyn seriously underestimated the diameter of the stellar system. He was so struck with the perfect symmetry with which, as his measures showed, the globular clusters were distributed above and below the system of the stars, that he felt that the diameters of the two systems were probably the same. In other words he considered that the bun was thoroughly buttered over its entire cut surfaces, and that the sun with the earth and the rest of the solar system was eccentrically placed in this layer about half-way to the bun's edge. As we shall see, Dr. Shapley was probably correct in this conception of the model, though he made it larger than it should be. In fact the investigations of the next fifteen years were to bring about a considerable increase in the diameter of the stellar system and a considerable diminution in the diameter of the system of globular clusters.

It will be noticed that this conception of Dr. Shapley placed the earth and the solar system not only eccentrically within the system of globular clusters but also eccentrically within the flattened stellar system. In all the statistical investigations it had been assumed that the solar system was more or less in the centre of the stellar system. This assumption had been made solely for the reason that the problem would otherwise have become quite unwieldy. In favour of our eccentric position in the stellar system now put forward by Dr. Shapley, there was already a good deal of evidence. For example the southern parts of the Milky Way, in the direction of the great star-clouds of Sagittarius, are far more brilliant and densely packed with stars than the other parts of it; and Dr. Shapley showed there could be little doubt that in this direction the stellar system extended further than in any other direction, and that somewhere towards these densest of the star clouds, or possibly even beyond them, lay the centre of our sidereal system.

We must now return to the spiral nebulae. It will be remembered that we left them about the year 1918, just at the point at which the investigations of Dr. van Maanen seemed to have provided a *reductio ad absurdum* to the island universe theory. During the years that followed the announcement of this apparently final result, Dr. Slipher continued at the Lowell Observatory photographing the spectra of the spiral nebulae for the determination of their radial velocities. By 1924 he had increased the number of these objects for which he had obtained the necessary data to nearly forty. His results were now even more startling than his earlier conclusions, for it appeared that some ninety per cent. of these objects were rushing away from us, and at speeds extending in some cases to more than 1,000 miles a second. In the meantime Einstein's theory had been put forward; and a modification of it due to the late Dr. de Sitter had indicated that very distant objects in space should be receding from us rapidly. This theory it was suggested, could provide the only rational explanation of Dr. Slipher's results, but it would necessarily carry with it the implication that the spiral nebulae were excessively remote. It certainly did not seem to harmonize with the previous conclusions of Dr. van Maanen; and it undoubtedly stimulated the island universe theory, recently thought to be dead, to show some signs of revival.

But the most exciting phase in the whole history of the investigation of the nebulae came in 1925. In an important paper, Professor Lundmark, then Director of the observatory at Upsala in Sweden, discussed at length the available data relating to the distances and motions of these objects. He found a definite correlation between the velocities with which they were rushing away from the earth and their apparent diameters on photographs. The smaller they were the more quickly they rushed, and since it appeared likely that, as in the case of the globular clusters, their diameters were at least a rough indication of their distance, he concluded that the velocities with which the spiral nebulae were receding were greater the further away they were. And this was just what had been predicted by the relativity theory. Of still greater interest were his conclusions on the rotation of these objects. During a visit to America he had gone to Mount Wilson and remeasured some of the photographs originally measured by Dr. van Maanen, using moreover the same measuring machine. The results he had got were completely different. He found the rotation to be about ten times slower than that found by Dr. van Maanen, and was forced to the conclusion that the technical difficulty in measuring such ill-defined objects as the condensations in the arms of the spiral nebulae was so great that the results could not be relied upon. But the most remarkable part of the paper dealt with a number of " new stars " that had been photographed in the spiral nebulae. From the evidence available as to the distances of the " new stars " in our own system it seems that their candle-power at maximum brilliance reaches a level which within certain limits is the same for all " new stars." On the assumption that the " new stars " photographed in the spiral nebulae were essentially similar to those found in the stellar system and rose to the same sort of candle-power, Professor Lundmark found that the nebula in Andromeda, which is the brightest of the spiral nebulae, was about one and half million light-years distant.

But this was not all, for practically at the same time there came from America news that almost identical results had been arrived at independently and by entirely different means. Dr. Edwin Hubble with the 100-inch telescope at Mount Wilson had made a laborious search in the Andromeda nebula, and in another spiral nebula, for Cepheid variable stars. His search was successful; and from a study of the periods and the apparent brightnesses of the stars which he found, he deduced, in the same way as Dr. Shapley had done in the case of the globular clusters, the distances of these spirals from the earth. They each turned out to be at a distance just short of a million light years. Considering the enormous distances involved the agreement between the results obtained by these two different methods of investigation was very remarkable. There could be no doubt that the distances were of the right order of magnitude, and that the spiral nebulae were placed far outside both the stellar system and the system of the globular clusters. So henceforward the theory of the island universes was by no means dead.

These earliest measurements of the distances of the two brightest spiral nebulae raised immediately a number of interesting questions. Were the fainter spiral nebulae still more remote? If so, to what distances did they extend and what was the total number of them? What was their nature, and, if they were really systems of stars, how did they compare in size and structure with our own stellar system? Did the tendency to rush madly away from the earth apply to all the spirals as it did to the majority of the nearer ones, and if so why?

Even before he had published the results just referred to, Professor Hubble had started trying to discover the answers to these questions. In his first investigation he had discovered and made use of twelve Cepheids in the Andromeda nebula. Later he increased this number to about forty, all of which supported his first conclusions as to their distance. Most of these were Cepheids with long periods going through their light cycles in from eighteen to fifty days. The period-luminosity law shows that these stars have candle-powers several thousand times that of the sun, and yet in the Andromeda nebula they appear 150,000 times fainter than the faintest star visible to the naked eye. To reduce several thousand times the sun's light to such a degree of faintness requires that the nebula in question should be about a million light-years away. In about half a dozen more spirals he also found Cepheids which gave him their distances; but all the other spirals were so remote that even the longest exposures with the 100-inch telescope failed to reveal the Cepheids within them. Fortunately in the spirals near enough to show Cepheids there were numerous other stars considerably brighter. The candle-power of these brightest stars was easily measured—since their distances were now known—and found to be about 60,000 times that of the sun and about equal to the brightest stars known in our own stellar system. He could therefore assume that the brightest stars in the spiral nebulae were of this particular candle-power, and could use them to get the distances of spirals which were too remote for their Cepheids to be visible. This brought another thirty or forty spirals within reach of his sounding line, and carried him out to a distance of six million light-years.

The number of the spiral nebulae had long been known to be enormous. In 1899 Keeler at the Lick Observatory had estimated the number within reach of the Crossley reflector as at least 120,000. More recently extensive surveys of these spirals had been made all over the heavens, especially at the Harvard Observatory and at its stations in the southern hemisphere. They had been undertaken with photographic instruments of moderate size, capable of portraying comparatively large regions of the sky with considerable speed. And their purpose had been to study the brightness of these objects and their distribution in the heavens. We have already referred to the absence of the spiral nebulae from the immediate neighbourhood of the Milky Way, and we pointed out that this was one of the chief objections to the view that they were systems independent of our own. The new photographic surveys had fully confirmed this apparent anomaly and brought to light two other important facts. First, although like the globular clusters they completely avoided the belt of the sky containing the Milky Way and the regions immediately bordering it; they were, unlike the globular clusters, concentrated most thickly of all in the two parts of the sky furthest from the Milky Way. Secondly, it was found that the spiral nebulae were frequently collected together in clusters, some of which contained several hundred nebulae packed closely in a small area of the sky.

(At this point it will be useful to introduce a term which so far has been avoided for fear of confusion. The word *galaxy* was originally used purely as an alternative for Milky Way; it simply referred to that luminous band of light which encircles the heavens. It then came to be used as a synonym for the stellar system, the flattened disc-like system containing the stars, open star-clusters and gaseous nebulae, but not including the globular clusters and spiral nebulae. Now, however, since the general acceptance of the island universe theory, the word *galaxy* may be used in reference not only to our own stellar system but also to individual spiral nebulae, each of which is to be regarded as a separate stellar system or " galaxy." Our own stellar system is spoken of as " *the* galaxy " or " our own galaxy " to distinguish it from any other galaxy such as the great nebula in Andromeda. More recently Professor Shapley has introduced two additional terms which are in general use. He calls a cluster of spiral nebulae a *super-galaxy*; for example the " super-galaxy in Virgo " is a cluster of over 500 nebulae. Finally he refers to the sum total of the spiral nebulae as the *meta-galaxy*, or the *meta-galactic system*.)

So far Professor Hubble had measured the distances of some forty nebulae up to distances of about six million light-years. The next problem was how to extend his sounding line so as to cope with the still more distant nebulae in which not even the brightest stars could be photographed. This he very ingeniously managed to do with the aid of the clusters of nebulae, particularly of the great cluster in Virgo.

The super-galaxy in Virgo with its population of 500 nebulae is sufficiently close for some of its members to contain a few stars bright enough to be photographed. The distance of those which contain stars can thus be measured; and since the group is a most compact one we know that all the other nebulae in it must be at the same distance. On examining these 500 nebulae Professor Hubble found them to be very much of the same brightness, in fact the great majority fell within the limits of one-half and twice the brightness of the average brightness of the lot. He then examined twenty other super-galaxies similar to the one in the constellation of Virgo and found that in them also the brightness of the various nebulae fell within the same narrow limits. For example, the hundred nebulae forming the super-galaxy in Pegasus had an average brightness equal to one-sixteenth of the average brightness of the nebulae in the Virgo cluster; but again the great majority of those hundred nebulae lay between one-half and twice the average brightness of them all. The obvious conclusion to be derived from these results was that all the spiral nebulae had very closely the same total candle-power; and that therefore the relative average brightness of the nebulae in the various super-galaxies gave a measure of their relative distances. This meant that since the nebulae in the super-galaxy in Pegasus had an average brightness one-sixteenth of the average brightness of the nebulae in the super-galaxy in Virgo, the former super-galaxy must be four times as far away as the latter, and therefore at a distance of twenty-four million light-years.

In this way the distances were found for the twenty known super-galaxies containing in all several thousand nebulae. They extended to distances of over one hundred million light-years. From these results, and especially from those of the Virgo cluster which alone had been derived from the observation of actual stars, the average candle-power of a spiral nebula was found to be about a hundred million times that of the sun. As we saw, the great majority had candle-powers between one-half and twice this value; but very occasionally there would be a very faint nebula with one-tenth of that candle-power, or a very bright nebula with ten times that candle-power. And so while the determination of the apparent brightness gave one a very accurate measure of the distance of a *cluster* of nebulae or of a *statistical group* of nebulae, it merely told one in the case of an *isolated* nebula that its distance was *probably not more than fifty per cent. greater or less than a certain figure.*

Directly Professor Hubble had got the distance of a spiral nebula it was a simple matter to work out how large it was. He simply had to calculate how big it would have to be in order that, at its actual distance, it would appear as big as it looked. He found the Andromeda nebula to have a diameter of 45,000 light-years, which although very large is small in comparison with the diameter of 260,000 light-years found by Dr. Shapley for our own galaxy. Moreover, some of the other spiral nebulae appeared to be yet smaller. This discrepancy between the dimensions of our own galaxy and those of other galaxies came naturally as something of a disappointment after the wave of enthusiasm for the island universe theory that had swept over the astronomical world in 1925. It had immediately been presumed that every spiral nebula was a galaxy built roughly to the same pattern and on the same scale as our own galaxy. There were of course explanations. As Professor Shapley once put it, " if we call them *islands*, our own galaxy is a *continent*." Yet it seemed a retrograde step to suppose that the galaxy we inhabited should just happen to be the largest of them all. A more acceptable view, also considered by Professor Shapley, and subscribed to in slightly different form by Professor Lundmark and by Dr. Trumpler of Mount Wilson, held that the galaxy with its star-clouds, the globular clusters and the Magellanic Clouds formed in reality a composite group of galaxies similar to some of the smaller of the clusters of spiral nebulae. However, even this last explanation was not completely satisfactory. If our galaxy was really a *group* of galaxies, then we should have to suppose that all those of them which made up the Milky Way were arranged in a flattened disc-like system. But we know of no cluster of spiral nebulae arranged in such a way. This apparent discrepancy between the dimensions of the galaxy and those of the spiral nebulae was later to receive a satisfactory but quite different explanation.

With regard to the composition and structure of the spiral nebulae, it had long been known that these objects gave spectra similar to that of star-light and so were presumably composed of stars too far off to be individually distinguished. And now more recently the 100-inch telescope had revealed in the nearer spirals such as the Andromeda nebula large numbers of separate stars. In addition one could see in the photographs of the spirals numerous condensations of greater brilliance strongly resembling the star-clouds of the Milky Way. The spirals which happened to be turned edge-wise to us in the sky were seen to be highly flattened systems, very shallow in comparison with their diameters—again suggestive of the Herschel, Seeliger and Kapteyn models of our galaxy. In many of the photographs of these edge-on spirals one saw curious dark rifts extending around their waists, so that they looked like the two halves of a split biscuit slightly separated. It was impossible to doubt that these dark rifts were produced by clouds of obscuring matter similar to those dark nebulous clouds which Barnard's photographs depicted in such numbers against the starry background of the Milky Way. Finally, with the largest instruments it had been possible to detect in isolated portions of some of the spirals the bright spectral lines of glowing gases which demonstrated the presence there of gaseous nebulae like those in our own galaxy. So everything seemed to conspire together to emphasize the similarity of the spiral nebulae to our own stellar system—everything except the scale on which they were built, which appeared to be so unaccountably different.

The other urgent question that required an answer was whether the tendency found among the nearer nebulae to rush away from the earth belonged as a general rule to all spiral nebulae. The work done so far on this problem was almost entirely due to Professor Slipher at the Lowell Observatory. But he had now studied all the spirals that could be reached with his instruments; and if the investigation was to be carried any further the only telescope large enough for the purpose was the 100-inch at Mount Wilson. Of late a fresh reason had cropped up why this investigation should be extended to the most faint and distant nebulae. As we saw, Professor Lundmark, from a discussion of the diameters of those spirals of which the velocities had been measured, had obtained fair evidence to show that they receded more rapidly the more distant they were. One form of the relativity theory had predicted that the distance and velocity of remote bodies should go hand in hand in precisely this way; and so it had been suggested that the relativity effect was to be identified with the recession of the nebulae. It was therefore extremely important to see whether this distance-velocity relation would continue to hold good for nebulae still more remote than those which Professor Slipher could reach. And so in 1929 Mr. Humason started on a programme for photographing the spectra of increasingly fainter and more distant nebulae with the great reflector on Mount Wilson.

The distance of the furthest nebula whose velocity had been measured by Professor Slipher was about ten million light-years. By 1931 Mr. Humason with the 100-inch had extended the measurements to a distance of 150 million light-years. At first he only used nebulae which were members of super-galaxies, so their distances were all known with a high degree of certainty. By comparing these velocity measurements with his own measurements of distance Professor Hubble was able to show that the suspected relation between distance and velocity held good with extraordinary exactness for all these nebulae. Their velocities increased regularly by one hundred miles a second for each million light-years of distance. Thus a nebula at ten million light-years would be receding at a speed of one thousand miles a second, while one 150 million light-years distant would be receding at a speed of fifteen thousand miles a second. As to the existence of this curious relation there could be no doubt, for it was based on a study of a very large number of nebulae. The only exceptions to it were half a dozen objects among the brightest and nearest of the nebulae. And even these exceptions were probably more apparent than real. Already evidence was accumulating to show that our own galaxy was in rapid rotation; and it was probable that in certain directions in the heavens our own velocity round the galactic centre would more than neutralize the recessional velocities of the nearer spirals.

CHAPTER X

THE DIAMETERS, COLOURS AND TEMPERATURES OF THE STARS

In 1920 a most remarkable instrument was brought into use with which it was possible for the first time to measure directly the diameters of the largest stars. It is not always realized by those who are not astronomers that no telescope however big can make a star look any larger than it looks to the naked eye. In fact the larger a telescope is, the smaller it makes a star appear. One effect of a telescope is to diminish the distance between ourselves and the thing we are looking at. The use of a telescope which magnifies 300 times is equivalent to dividing the distance which separates us from the object by 300—at least as far as the apparent *size* of the object is concerned. When we look at the sun, the moon, the planets, the star-clusters and the nebulae we see them as quite large objects in the field of view of the telescope; for these bodies are near enough or sufficiently enormous to cover an area of the sky of appreciable size. But the stars themselves are so immensely more distant than the planets and so very much smaller than the clusters and nebulae, that with our largest telescopes and greatest magnifying powers they still appear as points of light without any perceptible size or shape.

The large lens at the end of a telescope collects the light from the object and forms it into an image. The eye-piece simply plays the part of a magnifying glass and is used for magnifying the image. The eye-piece is interchangeable; and within limits one can magnify the image as much as one likes. Most of us know to our cost that we cannot go on enlarging a photograph with impunity. If we enlarge its size more than two or three times we bring out nothing new except the grain of the plate. Now we may regard the image formed by a telescope lens as possessing a " grain " somewhat analogous to the grain of a photograph, except that it is very much finer. The detail we can detect in an image is limited by the size of its grain, and since, as we shall see, the size of the grain depends on the size of the main telescope lens—being smaller, the bigger the lens—, a big telescope is capable of revealing finer detail in an object than a small telescope. This is also the reason why it is futile to employ excessively high magnifications on a telescope, and why one can use with advantage a higher magnification on a large telescope than on a small one.

What happens if one looks at a bright object which is so small or so remote that its image is actually smaller than the optical grain of the telescope being used? The answer is that one sees what one may consider as an individual grain: one sees something which has the size and shape not of the object but of the grain. The shape of an individual grain due to a telescope lens is perfectly characteristic: it is a small round spot of light surrounded by a number of concentric circles of diminishing brightness. The size of the grain—the diameter of the central spot and of the rings—is, as can be shown from the theory of optics, in inverse proportion to the diameter of the telescope lens. Now there is no star in the sky large enough or near enough to give even in the 100-inch telescope an image as big as the excessively fine grain of that instrument. So all stars appear the same in all telescopes, tiny discs of light surrounded by bright circles, and the only variation is in their size (or rather the size of the grain) which diminishes as the telescope gets bigger. The image of a star in a telescope is thus a "spurious image"; it is produced by the interference of light waves, and its shape has nothing to do with the shape of the star itself.

It has long been known from theory and experiment that if one looks at a star with a telescope, the lens of which is covered by a card with two openings in it, one will see the spurious image of the star crossed by a number of evenly spaced dark bands. The spacing of these dark bands depends only on the distance between the two openings in front of the lens, the spacing of the bands being made wider as one brings the openings closer together. If the openings are arranged so that the distance between them is adjustable the spacing of the bands can be varied at will. It was found that this method could be used for measuring the apparent separation of two stars which were so close together that they could not be distinctly separated in the telescope. The principle is this. If two stars are very close together their spurious images will almost completely overlap and yet not quite coincide. If we start with the openings close together and gradually move them apart, the bands in each of the spurious images will be brought closer and closer together until a point is reached when the dark bands of the one will be obliterated by the bright parts of the other. The distance between the openings at which the dark bands vanish will give a measure of the apparent distance in the sky between the two stars. Of course if the stars are very near to each other the lens of the telescope may not be big enough to give a sufficient separation between the openings. But the interesting *thing is that by this method one can measure the separation between* two stars with a telescope which is about half the size of one that is necessary for *seeing* the stars separately.

But the method is not confined to measuring the apparent separation of two stars, it can also be used for measuring the apparent diameter of a celestial object provided its image is just larger than the optical grain of the instrument. It is not so easy to see why this is so as it is to see how the separation of two stars can be measured. We may regard the disc of the celestial object as composed of two half-discs in apposition to one another, and we may consider the light from each of the half-discs as concentrated at their respective centres of area. The little disc is then equivalent to two stars the separation of which is equal to half the diameter of the disc. Whether this argument is convincing or not, the fact remains that as far as this particular instrument is concerned the disc does behave as if it were two stars separated by half the diameter of the disc.

This method for measuring the apparent separation of two stars or the apparent diameter of a celestial object is called an " interference method "; and the device consisting of two moveable openings in front of the telescope lens is called an " interferometer." The application of the method to astronomy was first suggested on theoretical grounds by the French optical physicist, Fizeau, in 1868. In 1890 the theory was worked out in detail by Michelson in America. And during the next ten years Michelson with a 12-inch refractor at the Lick Observatory and Hamy at the Paris Observatory applied the method to the measurement of the apparent diameters of Jupiter's four main satellites. The images of these four objects are a little larger than the optical grain of a telescope about six inches in diameter and thus just show with such an instrument their proper shapes and their proper apparent sizes.

After 1900 the method lapsed into disuse. For although it possessed certain technical advantages over the ordinary method it did not achieve anything of which the older methods were incapable, while its application was definitely more limited. The classical method of measuring the apparent diameters or the apparent separation of celestial objects depended on the use of the *filar micrometer.* This was an instrument attached to the eye-end of the telescope in which two very fine, straight and parallel spider webs could be moved apart until they bisected the two images of the double star or lay upon the opposite edges of the disc of the celestial object under examination. The separation of the webs was then read off on the graduated " head " of the screw which controlled their movements, and so one obtained the apparent separation of the double star or the apparent diameter of the celestial object which was being measured.

In 1919, after an interval of over twenty years, Michelson again turned his attention to the astronomical application of the interferometer. He had since discovered that the separation of the two adjustable openings of the instrument need not necessarily be limited by the diameter of the telescope. If this idea turned out to be practicable and if the " openings " in the instrument could really be moved apart almost without limit, then the application of the method to astronomy would immediately become of the greatest importance. We have seen that even in the largest telescopes the images of all stars were smaller than the optical grain of the instrument. There was reason to suppose, however, that a comparatively slight increase in the diameter of the telescope would reduce the size of the grain below that of the images of a few of the nearer of the largest stars. If one could make a telescope about twenty feet in diameter—rather more than twice the diameter of the existing 100-inch—it ought to be possible to see the *real* discs, the real size and shapes, of a small number of stars. Or, if Michelson's new idea was practicable, a separation from ten to twenty feet between the openings in front of the 100-inch telescope, should result in the disappearance of the dark bands, and in a determination of the diameter, of just a few of the stars.

So in 1920 Michelson with Dr. Pease of the Mount Wilson Observatory designed and built an interferometer for attachment to the 100-inch telescope capable of a 20-foot separation of its openings. It consisted of a steel girder over twenty feet long placed across the mouth of the 100-inch telescope. The girder carried two fixed mirrors a little to either side of its centre, and two movable mirrors one at each end of the girder which could travel to and fro between the extreme ends of the girder and the two fixed mirrors. Each of the outer movable mirrors receives light from the star to be examined and reflects it inwards along the girder to the corresponding fixed mirror. The fixed mirrors then direct the light downwards into the telescope. The two movable mirrors take the place of the two openings in the original form of the instrument but have the advantage that the distance to which they can be separated is limited not by the diameter of the telescope but merely by the length of the girder. But the difficulty in making and working the instrument lies in the extreme precision with which the two sets of mirrors have to be lined up and in the extreme accuracy with which that alignment has to be maintained when the mirrors are moved from one position to another.

It was on July 10, 1920, that the 20-foot girder was first placed across the mouth of the 100-inch telescope, but it was not until December 13 that success was finally attained. The dark bands from a star were first seen in August, but it sometimes took as much as half the night to find them; and when the separation of the mirrors was altered it often took several hours' work to bring the bands again into view. During the next three months various improvements were made in the control and adjustments of the mirrors. On the memorable night of December 13 a plank was placed across the mouth of the telescope so that an assistant could sit upon it and make the final adjustments of the mirrors by hand. The first star they turned on was Algol but they could not see the bands. At that moment the eye-piece with which Dr. Pease was working fell out; he replaced it by another and the bands were immediately visible. Dr. Pease gives this as a simple rule for finding the bands: " Just drop an eye-piece on the floor and substitute another." The mirrors were then moved to a 10-foot separation and the bands were easily found again both on Algol and on Bellatrix, a star in Orion. But when they turned the telescope on the star Betelgeuse, the brilliant ruddy star in Orion, the dark bands had completely disappeared and no alteration in the fine adjustment could bring them back. That this was indeed a real disappearance of the bands was proved by turning the telescope back on other stars when the bands immediately returned to view. It was now midnight and the observers adjourned for cocoa. Suddenly Dr. Anderson, who was helping Dr. Pease, said, "It's a whale of a thing—let's see, as big as the orbit of Mars." The 10-foot separation of the mirrors at which the dark bands from Betelgeuse had disappeared indicated that this star had an apparent diameter equal to that of a half-penny at a distance of fifty miles. The distance of Betelgeuse had previously been measured with considerable accuracy, so it was a simple sum in arithmetic to work out its diameter in miles. It turned out to have a diameter of 260 million miles, or 300 times the diameter of the sun. If without disturbing the relative positions of the planets we transported the entire solar system and placed the sun at the centre of this gigantic star, the earth would still be revolving well within the surface of the star while Mars would be only a little distance outside it.

Incredible as this result may sound it was not entirely unexpected. We saw how the work of Professor Hertzsprung and of Professor Russell had shown that there were two types of red stars: the red dwarfs, small and dense; and the red giants, extremely rarefied and of immense size. Moreover, as we shall see presently, methods had been worked out by which the diameter of a star could be deduced from its colour and its brightness, and already Sir Arthur Eddington had predicted a value for the diameter of Betelgeuse which was almost exactly in agreement with that found by the interferometer. This was in fact the reason why Dr. Pease had selected Betelgeuse as one of the first stars on which to try out the new instrument: so it was not entirely by luck that he landed a " whale " at his first catch. It followed from the giant and dwarf theory that any bright naked-eye star which was also red was bound to have an exceptionally large diameter. So following upon his success with Betelgeuse, Dr. Pease turned his attention to other bright and red stars. Two of these were found to be even larger than Betelgeuse; thus α Herculis was 400 times the diameter of the sun, and α Scorpii, or Antares, was no less than 450 times the diameter of the sun.

There are extremely few stars that are large enough and near enough to come within reach of the 20-foot span of this first interferometer. In order that the disc of a star may be detected and measured with this instrument its apparent diameter must be at least as large as that of a halfpenny at 120 miles. The three stars already referred to are of quite exceptional size and are known as red " super giants." Nevertheless there are a few ordinary red giants, with diameters from twenty to forty times that of the sun, which come within reach of the instrument because they happen to be very much nearer to us. Such stars include Arcturus and Aldebaran. Altogether seven stars have been measured in this way; and, except in the case of Aldebaran, the results are in good agreement with the diameters predicted from measurements of colour and brightness. In the case of Betelgeuse, which is a variable star, the interferometer shows that the diameter is also variable as though the star were pulsating. But whether the pulsation is synchronous with the light variation —as appears to be so in the case of the Cepheids—is not yet proved.

It next became desirable to increase the scope of the interferometer by building one with a still wider span. It was out of the question, however, to attempt to carry any heavier instrument on the 100-inch. We saw how the apertures in front of the mouth of the telescope had been ingeniously replaced by mirrors which could be separated beyond the limits of the mouth of the telescope; we shall now see that the next step was to do away with the telescope altogether. It was like the smile on the face of the Cheshire Cat which persisted after the cat and its face had disappeared. After all it had been a wasteful form of luxury to use a 100-inch mirror with the 20-foot interferometer; for all that was actually required was a portion of the mirror large enough to receive the two beams of light coming from the two small mirrors near the centre of the girder. But in the early experimental stage of the instrument it was reasonable to use a mounting that was already available, rather than make one specially for it; and the 100-inch was the only telescope in the world capable of carrying such a heavy attachment. So the new interferometer with a 50-foot span which has now been built is a very different sort of machine. Instead of being an interferometer attached to a large telescope, it is an interferometer with a small telescope attached to its middle. This instrument will be capable of measuring the apparent diameter of a star even though it be no larger than that of a halfpenny at a distance of nearly 300 miles.

We have seen that the results of the interferometer were not entirely unexpected, and that already on purely theoretical grounds the diameters of the stars could be predicted. Nevertheless, since a number of assumptions had to be made in the theory, its results could not be regarded as certain in the absence of experimental confirmation. Before the invention of the interferometer there was only one method for getting direct information about the diameters of the stars. As we saw on page 109, this could be done in the case of eclipsing variable stars which also happened to be spectroscopic double stars. The manner in which the light of such a star varied gave one a model of the eclipsing system—the relative diameters of the two stars and the orbit in which the smaller star revolved round the larger; while the manner in which the spectral lines oscillated back and forth, gave the scale of the model in miles. The diameters obtained in that way ranged from about eight and a half times the sun's diameter in the case of the white stars, to about half the sun's diameter in the case of the red dwarf stars. There were no red giant stars among the eclipsing variables that were also spectroscopic doubles, and so in just those cases in which theory gave its most startling results and the largest diameters—the very cases in which experimental confirmation was most required—observation had been unable to help. Hence the extreme importance of the interferometer: although it was limited to a mere handful of the nearest of the largest stars, it gave us just the information that we wanted most urgently.

We must now consider for a moment this matter of the theoretical investigation of the diameters of the stars from a consideration of their brightness, colour and surface temperature. The whole subject is highly complicated and difficult and we shall do no more than touch upon some of the general principles involved. It is a matter of common experience that if a body is hot enough to be luminous an alteration in its temperature produces a corresponding change in its brightness and colour. If a poker is gradually heated up in the fire it first of all glows with a dull red light. As it gets hotter its colour changes through orange and yellow to white, while at the same time it becomes brighter. Further, if while the poker is being heated up we examine it with a spectroscope, we find that its spectrum undergoes certain characteristic changes. When the poker is just aglow with a dull red light the red region of its spectrum is the brightest part. As the temperature of the poker rises the region of greatest brightness gradually shifts along the length of the coloured band towards its blue end.

In the ideal type of body, known as the "perfect radiator," there are certain well-known laws connecting the temperature both with the brightness and with the colour or spectrum. Stephan's law tells us how we can calculate the temperature of a body from the total amount of radiation (visible radiations of all colours, *plus* invisible ultra-violet radiations and heat radiations) emitted by each square inch or square centimetre of its surface. Planck's law tells us how we can calculate the temperature of a body from the relative brightness of different parts of its spectrum. And finally Wien's law, which is really a simplification of Planck's law, tells us how we can calculate the temperature of a body from a knowledge as to which part of its spectrum is brightest.

Now most bodies do not behave, even approximately, as perfect radiators, with the result that the above laws do not apply to them directly. But in the case of the sun and stars there is good reason to suppose that the laws of a perfect radiator apply very closely. Even so, however, there are serious complications. The light reaching us from the stars comes to us from different layers lying at varying depths in the atmosphere of the star, and these different layers are at *different* temperatures. Another complication is introduced by the earth's atmosphere which absorbs the violet and blue light from a star more than the red light, and thus makes a star look slightly redder than it really is and causes the red part of its spectrum to be disproportionately bright. Finally, there is the outer atmosphere of the star itself, which again absorbs light from different parts of the spectrum to very different extents. It follows that the detailed theory of the "surface temperatures" of the stars and the observational technique involved in their measurement are both highly complicated.

As long ago as 1906 Professor Hertzsprung pointed out that, if the stars could be regarded as perfect radiators, then the laws relating their surface temperatures with their spectra and with the brightness of each little piece of their surfaces, could be used to calculate their apparent diameters. The study of the spectrum of a star gives us (by means of Planck's law or Wien's law) a measure of its surface temperature. Stephan's law then tells us how brightly each little piece of the star's surface will be shining at that particular temperature. If the star happens to be comparatively close to us, so that we can measure its distance from us, we can work out as explained previously its candle-power, or the brightness of its *whole* surface. One then simply divides the brightness of each little piece (say each square inch) of its surface into the brightness of its whole surface and gets at once the area of the star's surface and hence its diameter. But as Professor Hertzsprung pointed out, his method had a still wider application; for even if a star's distance was not known one could still work out from its spectrum what its *apparent* diameter was.

Imagine two balls of iron of the same size heated to the same white-hot temperature, and suppose one of them is placed twice as far away from us as the other. The nearer of the two will have twice the apparent diameter of the other and therefore four times the apparent surface area. The total brightness of the nearer ball will thus be four times that of the further one. But the intensity of the light from the surface of the two will be exactly the same. That is to say if you measure the brightness of a portion of the surface which looks as big as a sixpence held at arm's length, it will be the same for the nearer ball as for the more distant one. Thus Stephan's law relates the temperature of a body not only with the amount of light emitted by each square centimetre or square inch of its surface, but also with the amount of light emitted from each little piece of its surface with an *apparent* diameter equal, say, to that of a sixpence held at arm's length. Suppose now we place a very small white-hot ball of iron so far away from us that it has *no* perceptible size to the eye and looks just like a bright point of light. We cannot measure its apparent diameter by comparing it directly with a sixpence at arm's-length, because its size is below the limit of perception. Instead, by examining its spectrum and applying Stephan's law we can calculate how bright it would look, if it were brought near enough for its apparent diameter to equal that of a sixpence at arm's length. At the same time we measure the total amount of light reaching us from it—that is to say its apparent brightness—and then, by dividing this second quantity into the first, we find that, if only we had strong enough eyes, the ball would appear so many times smaller than the sixpence at arm's length.

This is exactly what Professor Hertzsprung suggested should be done in the case of the stars; for, as we have seen, there was no star which had even in the largest telescope any perceptible size. In 1906 he published a formula connecting up the surface temperature and the apparent brightness of a star with its apparent diameter, and applied it to the calculation of the apparent diameter of the bright star Arcturus. Even at that early date, when the determination of stellar temperatures was very much in its infancy, the result which he got was in good agreement with that obtained directly fourteen years later by the Michelson interferometer.

The chief source of uncertainty in these theoretical calculations of a star's diameter lay in the difficulty of measuring the surface temperature. When Professor Hertzsprung first published his formula for the diameters, the surface temperatures had only been very roughly estimated for a few bright stars. But in 1909 Dr. Wilsing, Dr. Munch and the late Dr. Scheiner at Potsdam, and Dr. Nordmann at Paris, started upon the systematic investigation of this subject. The early measurements of stellar temperatures by the Potsdam and Paris observers depended on visual estimates of the relative brightness of different parts of the spectrum, and were thus confined to the visible portion of the spectrum. But the method was a tedious one, and in 1914 Dr. Rosenberg in Germany substituted a photographic plate for the eye, and determined the relative brightness of the different parts of the spectrum by measuring by eye the relative blackening in different parts of the photograph. In 1922 Dr. Coblenz in America used yet another method: he allowed the various portions of the spectrum to fall in turn directly upon a vacuum thermocouple, which is an instrument sensitive to all forms of radiant energy and not limited to the photographic and visual regions. But the method which has probably given the best results was that described by Professor Sampson, Astronomer Royal of Scotland, in 1923. The whole range of the photographic and visual spectrum is photographed on a panchromatic plate, and an automatic record is made of the relative density of different parts of the photographic plate by allowing a beam of light to pass through it and fall upon a photo-electric cell.

In all these methods the surface temperature of a given star is obtained by applying Planck's law to the particular distribution of energy found in its spectrum. But such a detailed study of a star's spectrum is only possible in the case of the brighter stars, and entails moreover a great deal of work. Fortunately there are two other methods which can be carried out much more rapidly and which are applicable to much fainter stars. These two methods depend on the measurement of the " colour-index " and the " effective wave-length " of a star.

An ordinary photographic plate is sensitive to the violet and blue light but not to the green, yellow, orange and red light. The eye on the other hand is most sensitive to yellow light. When we measure the brightness of a star photographically we are thus really measuring the brightness of the violet and blue portions of its spectrum. And when we measure the brightness of a star visually we are in effect measuring the brightness of the yellow part of its spectrum. It is clear, therefore, that if we consider these two sorts of brightness-measurement together, we get what is really a measurement of the colour of a star. For example, the redder a star is, the less will its light effect a photographic plate, and the more will its visual brightness exceed its photographic brightness. On the other hand, in the case of the white stars, in which the blue parts of the spectrum are brighter than the red, the photographic brightness will be relatively greater than the visual brightness. Arbitrary scales of visual and photographic brightnesses have been determined with great care and accuracy; and what is known as the "colour-index" of a star is a figure expressing the difference between its photographic and its visual brightness. Thus the colour-index is a definite *quantitative* measurement of a star's colour. Recently it has been found preferable to measure the visual brightness by a photographic method—although this may sound like a contradiction in terms. The photographic brightness is obtained with an ordinary plate sensitive to blue and violet light; while the visual brightness is obtained with a panchromatic plate and a yellow filter selected to give a colour sensitivity similar to that of the eye.

Long ago Father Secchi had pointed out the close parallelism between the colour of a star and its spectral type. And now, when it became possible to express the colour of a star quantitatively by means of its colour-index, the correlation between colour and spectrum became even more striking. When the surface temperatures of the brighter stars had been determined by the laborious methods outlined above—by measuring the relative brightness or the relative amounts of energy along the length of the spectrum—it was found that they ran so closely parallel with the colour-indices that the colour-indices by themselves could be relied upon to provide quite a good determination of the surface temperatures. The great advantage of this method lies not only in its speed and simplicity, but also in the fact that it is available however faint a star may be.

Like the measurement of the colour-index, the measurement of the so-called " effective wave-length " of a star is also a short-cut to the study of its spectrum. And whenever in science one makes use of a short-cut, it is always useful to have a second and independent short-cut as a check on the reliability of one's results.

If one puts an ordinary garden-sieve in front of the object-glass of a telescope and looks at a star one will see in the field of view a curious pattern. In the centre is the ordinary image of the star somewhat reduced in brightness. Stretching out in a line on either side of the central image, and also in a second line above and below the central image, are a number of equally spaced minute spectra of the star. The four spectra nearest to the central image —above and below and on either side of it—are the brightest, while the brightness of the others falls off as their distances from the central image increase. By employing a simpler type of sieve, in which there is only one set of wires parallel to one another, one gets rid of the cruciform arrangement of the spectra and sees instead a *single line* of spectra lying on either side of the central image. In each of these tiny spectra the blue end is nearest to the central image while the red end is directed away from the central image. The method is to photograph the pattern on a very small scale and to consider only the two spectra immediately on either side of the central image. Since the scale of the photograph is very small each spectrum is barely distinguishable from a point, and appears only very slightly elongated. The photographic plate is then put into a measuring machine; and by moving the two adjustable wires in the eye-piece of the machine so that they come to lie respectively on the most intense portions of the two spectra, one finds the distance between them. In a high-temperature star the blue portion of the spectrum is the most brilliant; and hence on the photograph the most intense portions of the tiny spectra will be those directed towards the central image and thus nearest to each other. On the other hand, in a cool star the red portions of the two spectra, which are furthest from each other, will be the most intense. It follows that the measured distance between the two images will depend on the colour and the surface temperature of the star, the distance being larger for red cool stars and smaller for hot white ones. The beauty of the method depends on the fact that one has two spectra of the same star *facing in opposite directions.* A small shift of the point of maximum intensity in one of them, say from right to left, is accompanied by an equal shift in the other from left to right. Hence the change of distance we measure is just double the shift in either, which means of course a twofold increase in accuracy. The method is called the measurement of " effective wave-length " because one is in effect measuring in which colour of the spectrum, that is to say in which wave-length of the spectrum, the light is most brilliant. The actual distances between the spectra depend on the spacing between the wires in the sieve (or " coarse grating " as it is usually called), on the telescope lens and on the type of photographic plate. But when these factors have been accurately standardized and allowed for, the effective wave-lengths obtained are found to run closely parallel to the colour-indices, and to give reliable determinations of the colours and surface temperatures of the stars.

It may seem an odd way of going about an investigation of a spectrum purposely to reduce it to so small a scale that it requires a measuring microscope to distinguish it from a point. However, in the first place, we are here only interested in the bright background of the spectrum and wish to ignore completely the numerous dark lines crossing it. On the minute scale used these lines are quite invisible and so do not bother us. What we want is the position of the brightest part of the spectrum, its " centre of gravity " in fact. And if we wish to avoid those laborious methods mentioned above for the detailed study of stellar temperatures, we can measure the centre of gravity of the photographed spectrum more accurately if it is made very small. In the second place, if we are going to measure the position of something we must have something else in relation to which its position can be measured. With the sieve or coarse grating this is immediately possible because what we have to measure is the distance between the two spectra on either side of the central image. If we were to use an ordinary spectroscope there would only be one spectrum of the star; and although we could put our finger upon its centre of gravity, there would be nothing in relation to which its position could be measured. [illegible] should have to photograph it on a scale large enough to show up the spectral lines and measure the position of the centre of gravity in relation to them—but that would merely lead back to the larger instruments, the longer exposures, the more tedious methods and the limitation to bright stars which it was the whole purpose of the use of the coarse grating to avoid.

It will be convenient at this point to summarize briefly what we have been discussing. On the assumption that a star behaves as a perfect radiator, the study of the distribution of the light in its spectrum will lead us, *via* Planck's law, to its surface temperature. Stephan's law then gives us its surface brightness—the brightness, that is, with which each little piece of its surface is shining. By Professor Hertzsprung's formula we can then calculate the apparent diameter of the star, and also—if the star's distance is known—its actual diameter in miles. Moreover in the case of fainter stars, which are too faint or too numerous for the detailed study of their spectra, one can obtain all these data with considerable accuracy by the simpler methods of measuring the colour-indices or the effective wave-lengths.

The calculation of the apparent diameter of Arcturus by Professor Hertzsprung in 1906 was prior to the development of the more accurate methods of temperature measurement we have been discussing. The figure he got at that time was about twice as large as that which the interferometer subsequently gave; but as soon as the improved temperature determinations were available Sir Arthur Eddington was able to arrive at theoretical values for the diameters of Arcturus, Betelgeuse and Antares which were in almost precise agreement with the corresponding interferometer measures. Moreover in 1922 Professor Hertzsprung was able to publish, from the available colour estimates of different kinds, the apparent diameters of no less than 734 stars.

Before dealing with the more recent developments in the measurement of stellar temperatures and with the significance of the results, we must consider for a moment the colour and the spectra of the stars apart from their bearing on the temperatures, surface brightness and apparent diameters of the stars. After all colour-indices and effective wave-lengths, though indirectly measurements of temperature, were primarily measurements of colour and spectral type. And these two stellar characteristics are in themselves of the greatest importance. We have already seen how colour and spectral type go closely hand in hand: how the characteristic spectral lines in a red star are due to actual chemical *compounds*, how those in a yellow star are due to metallic vapours, while those in a white star are due to gases like hydrogen and helium. If we wish to arrive at some understanding of the structure of the universe, it is obviously desirable to see whether colour and spectral type are related in any way to such things as the distribution of the stars in space, their velocities through space, their brightness, their tendency to light variability, etc., etc. And if statistical studies of that sort are to have any claim to reliability, it will be necessary to determine the colour and spectral type, not of an occasional star here and there, but of as many stars as it is possible to reach. Hence the enormous value of the colour-index and effective wave-length determinations, which can be made so much more rapidly and carried to much lower limits of brightness than a detailed spectroscopic examination.

We need only mention a few of the more important results that have come to light through these statistical studies. We have already discussed at length the important relation between colour or spectral type and the absolute luminosity discovered by Professor Hertzsprung and Professor Russell. It showed that the stars fell into two colour sequences: the giant sequence in which the average brightness for each colour was approximately the same, and the main sequence in which the brightness fell off with increasing redness as one passed from the white giants to the red dwarfs. Another remarkable relation was found to exist between the colours of the stars and their average weights. We saw how the weights of certain of the spectroscopic and eclipsing double stars could be measured. The results showed clearly that there was a regular falling off in weight as one passed from the white giants along the main sequence to the red dwarfs. For a long time evidence had been accumulating to show that the white stars were moving through space with exceptionally low velocities, while the highest velocities were to be found among the red dwarfs. At first this was thought to indicate that as a star cooled and grew older it gained speed. In 1911 Dr. Halm at the Cape Observatory pointed out that since the white stars were much heavier than the red dwarfs, it was more reasonable to correlate the average velocities of the stars through space with their weights rather than with their ages, which at the best were purely hypothetical. He showed that if one took the white giants and multiplied their average weight by the square of their average velocity and then did the same thing for the red dwarfs, the result was the same in both cases. But the quantity obtained by multiplying the weight of a body by the square of its velocity is the " kinetic energy " or the energy of motion of the body. Thus it appeared that the average kinetic energy of the sluggish and ponderous white giants was the same as that of the light and quickly moving red dwarfs. The discovery was of capital importance. There is a well-known law in physics—Maxwell's law of the "equipartition of energy"—which states that in a mixture of two or more gases the average kinetic energy will be the same for the molecules of each of the gases, the heavier molecules will move more slowly and the lighter molecules more rapidly. Moreover, if the gases are suddenly mixed together in a vessel one can calculate how long it will take for this equipartition of energy to come about—for it depends on the frequency with which the molecules collide with one another. Dr. Halm's discovery suggested that the stellar system could be regarded as a gas and the stars as the molecules of the gas, and also that the system had been in existence at least long enough for equipartition of energy to have taken place. We shall refer to this again when we come to consider the age of the universe.

There have been many other statistical investigations into the colour and spectra of the stars. For instance it is found that the Cepheid variable stars are all white or yellow, and that their colours are closely correlated with their periods of variation and their absolute luminosities. As one passes along the series of the Cepheids from the whitest to the yellowest, both the light period and the absolute luminosity increase regularly. Again, the so-called "long period variables" (to be discussed later), which do not possess the clock-like regularity of the Cepheids, are all red or even very red stars. There have also been found interesting relations in regard to the colours of the stars forming the components of certain types of double-star systems, and also in regard to the colours of the stars in various kinds of star-clusters.

Finally the colour or the spectral type of a star is of the greatest importance from a purely technical point of view. All measurements of the brightness of the stars—on which so much of our knowledge of astronomy depends—are influenced by colour and spectral type, the effects varying with different methods and from one instrument to another. It is only by having precise colour determinations of the stars that such effects can be duly allowed for. One must even allow for the colour of a star when making accurate measurements of its position in the sky. So one sees how fundamental the colour question is: it is far from being only of aesthetic interest to the popular star-gazer.

CHAPTER XI

ATOMIC THEORY AND STELLAR SPECTRA

A NEW theory of fundamental importance was published in 1920 by the Indian astrophysicist, Professor Saha of Calcutta. It furnished an independent method for investigating the surface temperatures of the stars and so supplied a valuable check on the method described in the previous pages. But much more important was its dramatic illumination of the whole problem of solar and stellar spectroscopy, and for this reason we must consider it in some detail.

In the pioneering days of stellar spectroscopy it was thought that the spectrum given by a star told us literally what chemical substances that star was composed of. Some stars consisted mainly of hydrogen, others mainly of iron and other metals, others again contained a large preponderance of chemical compounds, such as titanium oxide and cyanogen. In other words, it was thought that the stars differed from one another very considerably in chemical composition. This somewhat ingenuous interpretation failed however to explain the discovery by Secchi that the vast majority of the stars could be placed in one of four spectral types which passed by imperceptible gradations from one to another. It was curious that the stars could all be arranged in almost a single uniform sequence, from the white hydrogen stars at the one end to the red stars with their beautiful fluted bands (now known to be due to titanium oxide) at the other: why was there this sequence at all, why should there not be an almost infinite variety in chemical composition? The idea soon arose that the sequence of spectral types represented the march of stellar evolution. The view put forward by Lockyer was that the composition of a star changed during its life cycle as a direct consequence of the changing temperature. In the youngest red stars the temperature was low enough for certain chemical compounds to remain intact. In the yellow stars the rising temperature broke up such compounds and even disrupted some of the heavier and more complex of the atoms. In the white stars the temperature was so high that the vast majority of all the atoms were broken down into the simplest atom, namely, the hydrogen atom. Finally, as the star proceeded to cool and retraced its steps down the colour sequence, the falling temperature permitted the hydrogen atoms gradually to combine again and build up the heavier and more complex atoms. In a sense this theory was purely speculative. From the dimmest ages the alchemist's dream had been to change the baser metals into gold. But although the transmutation of the elements has only just now begun to be realized in the laboratory, it was not unreasonable for Lockyer to suppose that at the enormously high temperatures of the stars the same sort of process might be taking place. Huggins, however, held a different view. He also regarded the changing spectrum of a star during its life-cycle as the result of the changing temperature, but he did not regard it as necessarily indicating a change in the chemical composition of the star. The white hydrogen star need not differ in the relative proportion of its component atoms from a yellow metallic star: he suggested that the differences revealed by the spectroscope might be more apparent than real; that the high temperature of a white star favoured the detection of hydrogen absorption, while the lower temperature of a yellow star brought out more conspicuously absorption by metallic vapours. It is now known that neither of these theories was correct; yet in each of them there was a certain grain of truth.

Between 1911 and 1913 the modern conception of the atom was developed as a result of the work of Lord Rutherford and Professor Niels Bohr. The immediate consequence of this was a much fuller understanding of the significance of spectral lines. For our present purpose we need only give an outline of the main features of the Bohr-Rutherford model of the atom. There are known to be at least ninety-two chemical atoms. All these atoms are built up out of two sorts of elementary particles, called protons and electrons. All protons are identical and all electrons are identical. A proton weighs 1,847 times as much as an electron, so that the weight of an atom may be regarded simply as the weight of all the protons it contains added together. Although a proton is so much heavier than an electron it is probably much smaller than an electron; and the diameter of an electron is several hundred thousand times smaller than the diameter of the complete atom. Each proton carries upon it one unit of positive electric charge, and each electron carries one unit of negative electric charge. Therefore when a proton and an electron are associated together in an atom their equal and opposite electric charges cancel each other out. Every atom contains a central nucleus in which all the protons of the atom are embedded. With the sole exception of the hydrogen nucleus, all the atomic nuclei have electrons also embedded in them; but the number of electrons in the nucleus is always less than the number of protons. The electric charge on the nucleus will thus clearly be equal to the number of protons *minus* the number of electrons embedded in it. For example, if there are four protons and two electrons embedded in the nucleus—as is the case with the helium atom—two of the four positive charges will be neutralized by the two electrons, and the net positive charge on the nucleus will therefore be two. Revolving round the nucleus of an atom are one or more electrons. An atom is, therefore, like a miniature solar system: the nucleus representing the sun, and the revolving electrons the planets. Now all atoms in their normal state are electrically neutral; and since the nucleus invariably carries a net positive charge the number of revolving electrons will have to be equal to the net positive charge on the nucleus. Thus in its normal, electrically neutral state the helium atom with its nucleus of four protons and two fixed electrons will contain two revolving electrons. The hydrogen atom is the simplest and lightest of all the atoms. Its nucleus consists of one solitary proton unaccompanied by a fixed electron; the electrical neutrality of the hydrogen atom is therefore brought about by the presence of a single revolving electron.

Now since hydrogen contains only one proton, and since the weight of an atom depends on the number of protons it contains, it ought to follow that the weight of each atom is an exact multiple of the weight of the hydrogen atom. But this was found to be only approximately true. In the first place it was found that the hydrogen atom had very slightly more than a quarter of the weight of the helium atom. On the other hand oxygen which has sixteen protons was precisely four times as heavy as helium with its four protons. In fact it was shown that if one defined the atomic weight of helium as exactly four—which meant putting that of hydrogen slightly above one—the atomic weights of a great many atoms would come out as exact whole numbers. But even so, why should there be this discrepancy in the case of hydrogen, the simplest of them all? The explanation was finally given by Einstein's theory of relativity. The old ideas about weight and mass have had to be profoundly modified by this new theory. The mass of a body no longer depends only on the amount of matter in it, but also upon the energy of the body. When four protons—or hydrogen nuclei—come together to form the excessively stable helium nucleus, a certain amount of energy is lost by them, and their combined weight becomes slightly less than four times the weight of each one individually.

Yet even when allowances were made for the slight loss of weight which occurred whenever four protons consolidated themselves into a helium nucleus, there still remained some glaring departures from whole numbers among the atomic weights. Chlorine had long been known to have an atomic weight of about thirty-five *and a half*; and as the precision of the measurements increased, more and more exceptions to the whole number rule were revealed. It was not until 1923 that the puzzle was properly unravelled by Dr. Aston's discovery at Cambridge of what are called Isotopes. Dr. Aston found that many of the elements which were supposed to be composed of identical atoms were really mixtures of two or more sorts of atoms, identical in their chemical properties but differing in their atomic weights. The chemical properties of an atom are determined entirely by the revolving electrons, so two atoms which are isotopes of each other and therefore chemically similar must each have the same number of revolving electrons. They must thus each have the same net positive charge on their nuclei. For example, chlorine with its supposed atomic weight of thirty-five and a half was found to be a mixture of two types of chlorine atoms, one with a weight of exactly thirty-five and one with a weight of exactly thirty-seven. The first had thirty-five protons and eighteen electrons in the nucleus, and seventeen revolving electrons; the second had thirty-seven protons and twenty electrons in the nucleus, but again seventeen revolving electrons. Numerous isotopes have been discovered but the most interesting of all are the isotopes of hydrogen discovered just recently. Ordinary hydrogen consists of one proton in the nucleus and one revolving electron. " Heavy hydrogen " has a nucleus containing two protons and one fixed electron, so that its net positive charge is one just as in the ordinary hydrogen nucleus. Thus both heavy hydrogen and ordinary hydrogen have a single revolving electron. Since their chemical properties are similar, heavy hydrogen like ordinary hydrogen combines with oxygen to form water, or more correctly " heavy water."

The lines in the spectrum of an atom are entirely due to the motions of the revolving electrons. Take first the simplest case of the hydrogen atom. Though there is only one revolving electron there are many alternative orbits in which it may revolve round the nucleus. The possible orbits are determined by perfectly definite rules, known as *quantum conditions*, and an electron cannot pursue an orbit which is not so determined. If left to itself the atom assumes a " steady state," and the electron revolves in the smallest of the possible orbits at a certain definite distance from the nucleus. In this condition the hydrogen atom possesses a minimum amount of energy; when the electron is revolving in one of the larger orbits the atom possesses a larger amount of energy, the amount depending on the size of the orbit. It follows that if the electron is to jump from its smallest into one of the larger orbits the atom must be supplied from some outside source with the requisite amount of extra energy. This will happen when the atom absorbs energy from a ray of light passing through it. The sun's surface is continually sending out an intense stream of light. On its way to us it has to pass through the sun's atmosphere in which there are enormous quantities of hydrogen. The hydrogen atoms are able to absorb energy from the sunlight passing through them, with the result that the revolving electrons jump into one of the larger orbits. When revolving in one of the larger orbits the electron is unstable, and sooner or later it topples back again into the smallest orbit, either directly or by steps through the intermediate orbits. In so doing it gives up the excess energy of the larger over the smaller orbit.

What happens when a hydrogen atom absorbs energy from light passing through it? As one might expect the light is absorbed. But instead of there being a general absorption of the light, a general dimming of it, the absorption is confined to a particular very narrow portion of the spectrum—a narrow gap is produced in the spectrum, a gap which we call a spectral line. Conversely, when an atom gives up energy, when the electron falls out of a larger into a smaller orbit, light is emitted in the same narrow portion of the spectrum, and this time a bright spectral line is produced. But the question of greatest interest is in regard to the *position* in the spectrum of the line emitted or absorbed. The answer to this was given by Dr. Bohr when he applied the principles of Planck's Quantum Theory to his own modification of the Rutherford model of the atom. We have seen that light may be regarded as a wave-motion through the aether. All light waves travel through the aether at the same rate; but depending on its colour the wave-length of a light ray, or the distance between its successive wave crests, varies. Red light and particularly infra-red light have long wave-lengths; orange, yellow, green and blue light have shorter wave-lengths, while violet and ultra violet light have still shorter wave-lengths. Since the velocities of all these kinds of light are the same while their wave-lengths differ, it follows that the *frequency* with which their successive wave crests pass a given point must also vary. The frequency is clearly smaller for the red and infra-red light and greatest for the violet and ultra-violet light. By applying the Quantum Theory to the atom Professor Bohr showed that the *frequency* of the light emitted or absorbed when an electron jumped from one orbit to another must be exactly proportional to the amount of energy given out or taken up by the atom as a result of that particular electron jump. If this were so it would be expected that jumps between the larger orbits and the *smallest* orbit—involving big energy changes—would produce lines of high frequency nearer the violet part of the spectrum; while jumps between outer orbits and *intermediate* orbits such as the second and third orbits—which involved smaller energy changes—would give rise to lines of smaller frequency nearer the red end of the spectrum. These expectations were fully realized. The exact positions of the twenty-seven hydrogen lines observed in the sun were given by the theory when calculating the energy absorbed as the electron jumped between the *second orbit* from the nucleus and the various outer orbits. The theory also predicted a similar series of lines in the ultra-violet parts of the spectrum due to jumps involving the larger energy changes between the *innermost* orbit and the various outer ones. And finally it predicted a third series in the infra-red part of the hydrogen spectrum produced by smaller energy changes when the electron jumped between the outer orbits and the *third* orbit from the nucleus. Lines lying exactly in the predicted positions have been found by Dr. Lyman in the ultra-violet and by Dr. Paschen and Dr. Brackett in the infra-red.

Thus triumphed the Bohr theory of the atom and the riddle of the hydrogen spectrum was at once completely solved. But unfortunately the hydrogen atom with its solitary revolving electron was of exceptional simplicity, and with an exception to be referred to in a moment, the same methods when applied to the other atoms gave only very partial solutions.

We saw that as a result of Einstein's theory and of Dr. Aston's discovery of isotopes all the atoms could be regarded as being built up of hydrogen nuclei, compressed when possible into helium nuclei, together with fixed electrons and revolving electrons. After taking into account the slight loss of weight due to the consolidation of four hydrogen nuclei into one helium nucleus the atomic weight of any atom could be expressed as an exact whole number. Nevertheless it is important to realize that if one follows the table of atoms in their proper order the atomic weights do *not* increase one at a time. For example, the six lightest atoms are, in order, hydrogen, helium, lithium, beryllium, boron and carbon. Yet their respective atomic weights are 1, 4, 6, 9, 11 and 12.

Finally, the heaviest atom known, number 92, has an atomic weight not of 92 but of 238. Why the atomic weights increase in this somewhat irregular fashion is not precisely known. It is certainly not because there are still other atoms to be discovered which will fill in the gaps—for, as we shall see in a moment, these are not really gaps. The probable explanation is that only certain combinations of protons and electrons can form a stable type of nucleus. In every atomic nucleus as many of the protons as possible are combined together in groups of four with the addition of two electrons to form helium nuclei, while the excess of electrons and the one, two or three protons left over are combined with the nucleus more loosely. It is probable that the structure of atomic nuclei and the resulting sequence of atomic weights is governed by considerations of stability. But apparently, in many cases, one or more alternative nuclei are compatible with a particular number of revolving electrons—as is shown by the elements which consist of isotopes, and particularly by krypton and xenon which consist of no less than six and seven isotopes respectively.

Although the atomic weights do not increase regularly according to a definite rule, the number of revolving electrons goes up evenly by one for each successive atom. Hydrogen has one, helium two, lithium three, beryllium four, boron five and carbon six revolving electrons; finally, uranium, the ninety-second and heaviest atom, has ninety-two revolving electrons. This is why we know there are no gaps remaining to be filled by undiscovered atoms, for all the ninety-two elements have been isolated. This does not prove however that there are no unknown atoms heavier than uranium.

It is generally believed that the electrons of the more complex atoms are arranged in successive layers or shells of orbits, each shell having a much larger diameter than the one immediately inside it. There is good evidence to show that as we pass from the simplest to the more complex atoms the various shells, or layers, starting with the innermost one are gradually filled with electrons until their full complement is reached. The first shell holds only two electron orbits and is therefore completed by helium with its two revolving electrons. The second shell holds eight electrons in it. It is completed by the gas neon with its ten electrons, two in the inner ring and eight in the second. The third shell also holds eight orbits and is completed by the gas argon with eighteen electrons. The fourth and fifth shells can each hold eighteen more electrons, and the sixth shell another thirty-two. This sixth shell is completed with a very heavy gas, radon, containing eighty-six revolving electrons, two in the innermost shell, eight in each of the second and third shells, eighteen in each of the fourth and fifth shells, and thirty-two in the sixth. There are known to be six elements heavier than radon, and it is possible that there are more. These six probably contain their additional electrons in a seventh shell. It is interesting that these last six elements all have radio-active properties; that is to say they are unstable and are continually breaking down into less heavy atoms.

It has long been known that the chemical properties of the atoms—the manner in which they react chemically in the presence of other atoms—is largely determined by the number of electrons which they have in their outermost shell. This is the explanation of the remarkable periodic repetition of general chemical properties as we pass up the Table of Elements. All the atoms of which the outermost shell possesses its full complement of electrons are chemically inactive. These are the *inert gases*, helium, neon, argon, krypton, xenon and radon. Those whose outermost shell is just " one short " of an electron are the elements fluorine, chlorine, bromine and iodine, which again are very similar to one another in chemical properties. Yet another example of similar elements is provided by the series of alkalies, lithium, sodium, potassium, rubidium and caesium, all of which have, like hydrogen, a single electron in their outermost shell.

It is also found that the spectral lines are entirely produced by the electrons in the outermost shell of an atom, by their jumps between their normal orbits in the shell and larger orbits, or between a larger orbit and a still larger orbit. The reason for this is that when an electron in one of the inner shells indulges in a jump the energy change is so great that the frequency of the spectral line produced places it far beyond the ultra-violet region of the spectrum in the X-ray region of the spectrum, and this is quite out of reach of the spectroscope. It was not to be expected that the theory would be capable of explaining the spectra of the more complex atoms with the same degree of success as it had done in the case of the hydrogen atom with its single electron. Nevertheless it succeeded in explaining many of the general features of such spectra, particularly in the case of the alkalies in which there was but a single electron in the outermost shell.

The absorption or emission of a spectral line by an atom is an instantaneous phenomenon; it occurs as a result of the jump of the electron and ceases as soon as the electron has reached its new *orbit.* The lines which we observe in a spectrum are produced by vast numbers of atoms whose electrons are jumping back and forth innumerable times each second. The intensity of a spectral line—the darkness of an absorption line or the brightness of an emission line—thus depends on the frequency with which the corresponding electron jump takes place. If jumps between the first and third orbits occur much more often than jumps between the second and third orbits the spectral line produced by the former jump will be correspondingly much more intense than the line produced by the latter jump.

The bright line emission spectrum of a substance can be obtained in a variety of ways. The more volatile substances can be vaporized in the flame of a Bunsen burner. The more refractory substances can be volatilized in the electric furnace, or by striking an arc or producing an electric spark across terminals made out of the substance in question. Finally, the spectrum of one of the permanent gases is got by passing an electric discharge through a glass tube filled with the gas at a very low pressure. In one or other of these ways the atoms of the element being examined are supplied with the necessary energy for their electrons to indulge in jumps from orbits of lower energy level to orbits of higher energy level. But we have seen that an electron in a higher energy state tends spontaneously to jump back (within a few millionths of a second) to its lowest energy state. And it is during this spontaneous jump back that it emits the bright spectral line corresponding to the energy liberated in the jump. Now at any given instant the vast majority of the electrons are jumping back and forth between one of the higher energy states and the *lowest* energy state (i.e., the smallest orbit). Thus the brightest lines and often the only lines visible will be those corresponding to the various jumps to this lowest energy level. It is found, however, that as the temperature, or degree of excitation to which the substance is raised, is increased, the greater will be the proportion of atoms to have their electrons at a given instant in an orbit of higher energy. So by raising the temperature of the furnace, or by making use of the electric arc, electron-jumps into orbits of higher energy level become sufficiently numerous to produce lines which were not visible at the lower temperature.

The same principles apply to the dark line absorption spectra. If a ray of white light is allowed to pass through the flame of a Bunsen burner into which some sodium has been introduced and is then examined with a spectroscope, the only absorption lines produced will be those due to jumps of the sodium electrons from their *lowest* energy state to states of higher energy. If however, the flame is replaced by an electric furnace containing sodium at a much higher temperature one will begin to detect sodium absorption lines due to jumps of the electrons from orbits of somewhat higher energy.

In regard to the production of absorption spectra it is natural to ask why the electrons which are spontaneously jumping back from higher to lower energy levels do not emit exactly the same amount of light as the electrons, which are simultaneously jumping in the opposite direction, absorb. In other words, why are not the absorption lines obliterated by the simultaneous emission of the same lines? Suppose the sodium vapour in the flame or furnace is capable of absorbing eighty per cent. of the light in the position of a certain spectral line, the whole of that eighty per cent. will be prevented from entering the spectroscope. On the other hand a quantity of light exactly equal to that eighty per cent. will be simultaneously *re-emitted* by electrons in the other atoms jumping in the opposite direction; but this eighty per cent. instead of being emitted in a thin parallel beam towards the slit of the spectroscope will be emitted in all directions equally. Thus only a negligible portion of the absorbed eighty per cent. of light will be returned, and the absorption line will be just as intense as if the re-emission had never occurred.

We have just seen that as the temperature of a gas is raised the percentage of the atoms which at a given instant have their electrons in the higher energy states is increased. But always the large proportion of the atoms have their electrons at the lowest energy level. Thus the lines corresponding to jumps to and from the lowest level are always the most intense lines. They are therefore referred to as "the principal series." Lines due to umps to and from higher energy states are said to belong to the various "subordinate series." Sometimes the principal series given by an atom is in the far ultra-violet part of the spectrum which owing to the absorption by the earth's atmosphere cannot be observed in the light from the sun and stars. We could then only learn of the existence of such an element in the sun or stars by detecting the lines in one of its subordinate series. But the subordinate lines even at high temperatures are much fainter than the principal lines; so unless the element was present in great profusion it might reasonably fail to be detected. A good example is hydrogen. We saw that jumps to and from its smallest orbit—the jumps which therefore give rise to the principal series—produce lines discovered by Lyman in the extreme ultra-violet.

The strong absorption of ultra-violet light by air makes it impossible to photograph these lines except in the laboratory. The hydrogen lines found in the sun and stars extend through the visible spectrum and are due to jumps to and from the second smallest orbit. It must mean that there are enormous quantities of hydrogen in the heavenly bodies for this first subordinate series to be strongly marked in them. Further, the way in which the hydrogen spectrum becomes more and more intense as we pass to the hotter and whiter stars is in excellent agreement with the theory just explained, that a subordinate series of lines increases in strength as the temperature rises.

But the most important astronomical consequence of Bohr's theory was that there was an upper limit to the size of the largest orbit to which an electron could jump. If it jumped any further than this it jumped out of the atom altogether. The atom was then one electron short; moreover, it was no longer electrically neutral but carried one excess positive charge. In this state the atom is said to be "ionized." The theory predicted that the spectrum of an ionized atom should be quite different from the spectrum of the same atom when not ionized; but should resemble very closely the spectrum of the unionized atom coming next before it in the table of the elements. For example, helium with its two revolving electrons will have when ionized only one electron; its spectrum therefore ought to be very similar to that of hydrogen which also has only one electron. The spectrum is produced by the behaviour of the circulating electron, and the difference in the behaviour of the hydrogen electron and the ionized helium electron can only be ascribed to the difference in the net positive charges of the two nuclei. The hydrogen electron is held in its orbits by the attraction of a single net positive charge, the ionized helium electron is held in its orbits by a double net positive charge. Calculation shows that the arrangement of the lines in the spectrum of ionized helium should be similar to that in the hydrogen spectrum except that the helium lines are displaced bodily towards the violet end of the spectrum. When Professor Bohr worked out the positions of the lines of ionized helium they were found to correspond exactly to a series discovered by Pickering in 1896 in some of the hottest stars, and sixteen years later detected in the laboratory by Professor A. Fowler. Prior to Professor Bohr's theory this series had actually been ascribed to hydrogen.

This important relation between the spectrum of an ionized atom and that of the unionized atom next before it in the table of elements is not confined to the case of hydrogen and ionized helium. It applies to the whole series of elements and has been confirmed by observation in a great many cases. Magnesium has two electrons revolving in the third shell; when it has lost one of these by ionization it resembles sodium which has one electron in the third shell. The spectrum of ionized magnesium is found to be a replica of that of sodium, but displaced bodily towards the violet.

The energy required to ionize an atom is greater than that necessary for displacing the electron into the outermost orbit. The energy can be supplied to the atom by raising the temperature or by exposing it to a high degree of electrical excitation. Some atoms need much more energy than others to ionize them. Thus some atoms are ionized at the comparatively low temperature of the electric furnace, more become ionized in the electric arc, while some of the more refractory elements are only ionized in the electric spark. The spectra of the permanent gases are produced by electrical discharges in glass tubes containing them at a low pressure; but to ionize them a great deal of energy is required, and this is particularly so in the case of helium.

It must not be supposed that at a certain temperature or degree of electrical excitation *all* the atoms of a given element become ionized. As the temperature is gradually raised a point will be reached when the percentage of the atoms that are ionized will be large enough to make a few of the ionized lines perceptible. With increasing temperature the percentage of ionized atoms goes up, while the ionized lines grow stronger and more of them come into view. At still higher temperatures more atoms will be ionized than not ionized, and finally ionization will be practically complete.

An atom however may lose more than one electron and theoretically the process of electron-stripping may go on until the atom has no electrons left. But at each successive removal the process becomes more difficult and requires ever increasing amounts of energy. Doubly and triply ionized atoms have been detected in the sun and stars and produced in the laboratory. Just as a singly ionized atom has a spectrum resembling that of the unionized atom next before it in the table of atoms, so the spectra of doubly and triply ionized atoms resemble respectively the spectra of the ordinary atoms two and three places before them in the table. But since with each new degree of ionization the excess positive charge on the nucleus goes up by one unit, the energy changes involved in the electron-jumps increase correspondingly and the spectral lines are placed further and further towards the violet. It follows that comparatively few of the multiple ionized atoms will give lines that are in the observable range of the spectrum, and that when such lines do exist they are more likely to be the fainter lines of one of the subordinate series.

Second and higher degrees of ionization take place in exactly the way described for the first degree, except that they require higher temperatures or higher degrees of excitation. A second ionization may begin to take place before the first ionization is complete when there are still unionized atoms present. Again, it is a question of the percentage of the atoms that are in the different stages; the higher the temperature the less numerous are the unionized atoms and the more numerous are the ionized ones. At fairly high temperatures the majority will be singly ionized, at still higher temperatures the majority will be doubly ionized, and so on.

Suppose the temperature of a gas was raised sufficiently for single ionization to start taking place and the temperature was then kept constant, might we not expect more and more atoms to lose their first electrons until all the atoms were singly ionized? The reason why this does not happen, why at a given temperature the percentage of ionized atoms remains steady at a certain constant figure, is that the broken off electrons are continually colliding into, and re-combining with, ionized atoms to form again unionized atoms. In this way an equilibrium is set up by which in a given time the average number of atoms ionized is equal to the average number of unionized atoms which are re-formed. It clearly follows from this that the percentage of atoms which are ionized will not only depend on the temperature of the gas but also on its degree of rarefaction. On the one hand the higher the temperature the greater is the rate at which the atoms are ionized; and on the other hand the denser the gas the more frequently will collisions occur and the greater will be the rate at which unionized atoms are re-formed. The relative rates at which these two opposite processes are taking place will obviously determine the point at which the equilibrium is maintained and settle what at a given instant the relative proportion of ionized and unionized atoms will be. Thus for a given temperature the percentage of the atoms of a gas which are ionized will be greater the more rarefied the gas is.

It was from considerations of this sort that Professor Saha in 1920 developed his important theory relating the degree of ionization in stellar atmospheres to their temperatures and pressures. By applying the equations of thermodynamics to the atomic theory of Professor Bohr, he established a relation between the temperature and pressure of a gas and the percentage of its atoms which were ionized. Professor Russell showed how the resulting percentage would be modified when one considered, instead of a single gas, a mixture of gases. In principle, by measuring the relative strengths in stellar spectra of the lines of the various ionized and unionized atoms, one could calculate the relative proportions of these two sorts of atoms, and thus get either the temperature or the pressure of the star's atmosphere provided one already knows one of them. By taking the theory in conjunction with other investigations giving an independent estimate of either pressure or temperature, the necessary data would be available for finding whichever of these two might be required. The actual calculation, however, is beset with difficulties.

But even without any exact knowledge of pressures we may assume that they are roughly the same in all stars and thus get the relative temperatures of stars of different spectral type. It is found that Professor Saha's theory arranges the stars in exactly the same order of temperature as the methods described earlier in the chapter which depended on measurements of the brightness of the background of the spectrum rather than on measurements of the intensity of spectral lines. Thus the two methods afford a valuable check on one another; but Professor Saha's method could also be applied to the very hottest and to the very coolest stars to which for certain special reasons the other method was not applicable.

The application of Saha's theory to the determination of stellar pressures was investigated in great detail by Professor R. H. Fowler and Professor Milne in 1923 and 1924 in England and subsequently by Dr. Menzel and Miss Cecilia Payne at Harvard. The former found the pressure in a star's atmosphere to be about one ten-thousandth of the earth's atmospheric pressure at sea-level. The latter found a similar sort of figure for the bottom of a star's atmosphere, and a pressure of one thousand-millionth of an atmosphere in the topmost layers. Dr. Menzel and Miss Payne got their estimates for different levels by the ingenious method of studying separately first the subordinate lines of the ionized and unionized atoms, and then the principal lines of the ionized and unionized atoms. They suggest that since the great majority of the atoms are in process of absorbing the principal lines, our view *via* such lines through the sun's atmosphere can penetrate to only a short depth before it is obscured, and must therefore represent the state of affairs in the highest layers. On the other hand never more than a small percentage of the atoms are absorbing the subordinate lines, hence our view *via* these lines through the sun's atmosphere can penetrate to a far greater depth, and thus represents the state of affairs in the lower atmospheric layers.

The pressures they get are of course excessively low, for even the highest pressures found at the bottom of a star's atmosphere correspond to that of a pretty efficient vacuum in the laboratory. There is little doubt however that the figures are fairly reliable, for the same sort of extremely low pressures are found by quite independent methods in the sun's atmosphere.

But the most valuable application of the Saha theory was the beautiful interpretation it gave of the spectral sequence among the stars.

M Type Stars. These are the cool red stars whose temperatures, round about 3,000 deg. C., compare with the temperatures attained in the electric furnace and in the electric arc. Their spectra consist largely of the beautiful fluted bands produced by actual molecules, which at these temperatures are able to withstand dissociation into their component atoms. The majority of the other lines are lines of unionized atoms. Only a small number of lines due to ionized atoms are present, and these are atoms like calcium which are readily ionized at low temperatures.

K Type Stars. These are the orange stars with temperatures of 5,000 deg. C. Very few of the molecular bands persist and then only faintly; for the higher temperature has almost entirely dissociated them into their component atoms. The principal lines of the unionized atoms are still very numerous and strong. In addition the subordinate lines of the unionized atoms have increased in number and strength. The reason for this is that the increased temperature has raised an appreciable percentage of the unionized atoms to those higher energy states in which they can absorb the subordinate lines. The important subordinate series of hydrogen also begins at this stage to be conspicuous. The lines of a few more ionized atoms appear, and those of the ionized calcium atom actually reach their maximum intensity in these stars.

G Type Stars. These are the yellow stars of the same type as the sun with temperatures about 6,000 deg. C. The intensities of lines due to ionized atoms are greater relative to those due to unionized atoms in these stars than in the K-type stars on account of the higher temperature. The subordinate lines of hydrogen still grow in strength, for as yet the temperature is not high enough to produce in this gas any appreciable ionization. On the other hand, the lines due to the ionized calcium atoms, which reached maximum strength in the K stars, now begin to weaken. This fall in the percentage of singly ionized calcium atoms is due to an appreciable proportion of them becoming doubly ionized.

F Type Stars. These are the yellowish white stars with temperatures about 7,000 or 8,000 deg. C. The relative prominence of lines due to ionized atoms is still more pronounced than in the G type stars. Most of the lines of the ionized atoms are subordinate lines for the simple reason that the majority of the principal lines are associated with such large energy changes that they lie out of reach in the ultra-violet. Moreover, in these stars some of the subordinate lines of the ionized atoms have already passed their maximum strength owing to a second ionization of their atoms having set in. The lines of ionized calcium though still conspicuous continue to weaken. The reason why these calcium lines persist so long and are so strong is because they belong to the principal series of calcium, and as we have explained a vastly greater number of atoms take part in absorbing the principal series than take part in absorbing a subordinate series. The lines of most other ionized atoms have nothing like the same degree of intensity or persistence for the reason that such atoms give only subordinate lines in the observable range of the spectrum. On the other hand the strongest lines of the unionized atoms tend to disappear as we pass up the spectral sequence in the direction of increasing temperature. Oddly enough the only lines of an unionized atom, which compare with those of ionized calcium in strength and persistence, are the lines of hydrogen, which are only subordinate lines. This curious anomaly is only to be explained by supposing that hydrogen exists in the stellar atmospheres in far greater profusion than any other element. In these F stars the subordinate hydrogen lines are getting extremely prominent.

A Type Stars. These are the white stars with temperatures around 10,000 deg. C. They are generally called the " hydrogen stars," owing to the extreme prominence of the hydrogen lines which reach their maximum strength in these stars. The lines of ionized calcium have now weakened considerably. The lines of the ionized atoms of the metals, so numerous in the F stars, have generally become much fainter as a result of a second ionization which mainly produces lines out of reach in the ultra-violet.

B Type Stars. These stars are still " whiter," or blue-white in colour, and have temperatures round 15,000 to 20,000 deg. C. The hydrogen lines are now weakening, for at such high temperatures the hydrogen atom is at last beginning to be ionized. Lines of unionized helium are appearing, for even in its neutral state this atom only begins to absorb in the observable region of stellar spectra at very high temperatures. There are also lines due to ionized oxygen and nitrogen. Finally, the lines of the ionized metallic atoms which were fading out in the A stars have now practically disappeared. Multiply ionized atoms do not often, as we have said, give conspicuous lines in the observable part of the spectrum. In these stars however we find lines of doubly and trebly ionized silicon—which again is compatible with very high temperatures. The doubly ionized silicon lines reach their maximum intensity in the hotter of the B stars while the lines of triply ionized silicon come to maximum strength in the hottest of all the B stars.

O Type Stars. These stars, also very white or blue-white, have still higher temperatures round 30,000 deg. C. or even higher. They are divided into two groups, those with dark spectral lines and those with bright lines either in addition or alone. Taking those which show only dark lines we find the lines of hydrogen and unionized helium are weakening, while the latter are being replaced by lines of ionized helium. In the hotter O stars the lines of ionized helium reach a maximum and in still hotter ones they begin to weaken as a result of a second ionization occurring. It is to be expected that in the hottest stars of all there would be a complete absence of observable lines owing to the extreme ionization. In a few stars this limit is nearly reached and the lines remaining in their spectra are excessively faint. In fact, towards the end of 1935 Dr. Kuiper of Harvard reported a star in the spectrum of which there were no lines at all. But the majority of the hottest O stars exhibit the extraordinary phenomenon of emitting bright lines in their spectrum. These lines are due to hydrogen, ionized helium and doubly ionized oxygen and nitrogen. When we see bright lines superposed upon the bright background of a continuous spectrum it usually means that the atoms emitting them are at a higher temperature than are the atoms giving rise to the bright background. This would seem to imply that the atmospheres of these hottest O stars are hotter than their underlying surfaces. It is almost certain that the proper explanation is quite different and more complicated. Because of this, and also because it is by no means definite, we shall merely point out that it assumes an extreme degree of ionization of the atoms concerned and thus is compatible with other indications of excessively high temperatures. O stars with bright lines are called Wolf-Rayet stars.

It is interesting that the spectra of O stars with bright lines pass by imperceptible steps to the spectra of the gaseous nebulae. We shall discuss the spectrum of the gaseous nebulae and the relation of these objects to the O type stars when we come to the general consideration of gaseous nebulae.

In addition to this clear interpretation of the spectral sequence later developments of the Saha theory also gave an explanation of the small differences between the giant and dwarf spectra and of the differences in the ordinary solar spectrum, the spectrum of sun-spots and the " flash spectrum " observed at eclipses.

The body of a giant star is much larger and far less dense than the body of a dwarf star. The gravitational attraction at the surface of a giant star is therefore much weaker than at the surface of a dwarf. Consequently the atmosphere surrounding a star will be more compact and dense in the dwarf than it is in the giant. According to the theory ionization takes place more readily at a low pressure. As the temperature increases the various stages in ionization outlined above should occur in the low pressure giant stars at a somewhat lower temperature than in the dwarfs. This is found to be the case. If we take a giant and a dwarf of the same spectral type (as judged by its lines) we find that the temperature given by the bright background of the spectrum (as measured by colour-index, effective wave-length, etc.) is definitely lower in the giant.

But it is particularly in the study of the solar spectrum that the Saha theory has given the greatest help. It will be remembered that long ago Lockyer had found marked differences between the spectrum of the general surface of the sun and the spectrum of sun-spots. Whereas that of the sun's surface closely resembled spectra obtained from the electric spark, that of the spots resembled spectra obtained from the electric arc. And since the temperature of the arc is lower than that of the spark, he concluded that the spots were cooler than other parts of the sun's surface. His explanation was that at high temperatures many of the atoms were " dissociated " into simpler atoms. In the light of the ionization theory the observed differences are explained down to minute details. In general, the surface spectrum is that of a G star whereas that of the spots corresponds with the spectra of the cooler K stars. Broadly speaking ionized lines are weakened and unionized lines are strengthened in the spots. And the degree to which such strengthening or weakening occurs for different atoms or for different lines of the same atom is shown by the theory to agree in amazing detail with the data obtained in the laboratory as to the different amounts of energy required to excite the different atoms to their various energy states or to ionize them.

It will be remembered that at the eclipse of the sun in 1870, Young of Princeton photographed for the first time the so-called flash spectrum. The dark lines in the ordinary solar spectrum are due to the absorption by the gases in the sun's atmosphere of the light coming up from below. At a total eclipse of the sun, just as the moon completely obscures the sun's disc, there will be momentarily exposed round the advancing edge of the moon the shallow rim of the sun's atmosphere. A moment later this also will be covered by the moon, but a well-timed photograph will catch it before it goes. At this moment there will be no light reaching us from the sun's surface for its atmosphere to absorb; instead, we shall see the light which the atoms in the atmosphere are emitting themselves. For an instant, therefore, the dark line solar spectrum is " reversed " and is replaced by a beautiful array of coloured lines, a bright line replica of itself. If one manages to catch the flash spectrum of the lowermost layers of the sun's atmosphere it is found to be identical as regards the number and position of the lines with the ordinary solar spectrum. But in its upper layers the spectral lines are much scarcer and correspond in general to the atoms of the lighter elements.

At the eclipse of 1898 in India the first really first-rate photographs of the flash spectrum were got by Lockyer and Professor A. Fowler. From these they made the important discovery that lines due to what we now know to be ionized atoms are relatively much stronger than they are in the ordinary solar spectrum. Probably the most extensive work done on the flash spectrum is that of Professor S. A. Mitchell, Director of the Leander McCormick Observatory, Charlottesville, Va. Since 1900 he has travelled over 90,000 miles to observe nine eclipses. His very beautiful photographs of the flash spectrum have been obtained by rather an unusual method and have the great advantage that they enable one to measure the relative heights to which the various atoms in the sun's atmosphere rise above its surface. From such investigations the strengthening of ionized lines in the flash spectrum was established beyond doubt, while Professor Mitchell's measurements of the relative heights of the gases showed that the strengthening of these lines became more and more marked at higher altitudes. Now it used to be supposed that the strengthening of lines due to ionized atoms was an indication of increased temperature. Yet it was impossible to imagine that the upper reaches of the sun's atmosphere were hotter than its lower layers. With the advent of the Saha theory this apparent paradox was explained. It was not only temperature, but pressure also, upon which the degree of ionization depended; and in the case under consideration the differences in pressure should have the larger effect. This problem of the solar atmosphere was the first problem to which Saha's theory was applied.

Before leaving the question of the interpretation of stellar spectra we must notice that the question which twenty years ago was asked in every book on astronomy, " What are the stars made of? " should now be replaced by the much less exhilarating one, " What is a star made of? " We have seen that the new interpretation of the stellar sequence is that it is primarily a manifestation of a gradual temperature change and has little or no bearing on differences in chemical composition. Miss Payne at Harvard has recently looked with considerable care into this very matter. From an examination, in large numbers of stars, of the temperatures at which the various subordinate and ionized lines first appear to be visible, she concludes that in general all stars have the same chemical composition. If a particular element was unusually abundant in a certain star then we ought to find the subordinate or ionized lines of that element appearing in that star at a somewhat lower temperature than normal. Perhaps this result is a little disappointing and monotonous. But if we have succeeded in giving the reader even a small idea of the complexity of modern astronomy he will probably join with the astronomer in gratitude for this one really sweeping simplification.

CHAPTER XII

THE WHITE DWARFS AND THE INTERIOR OF A STAR

We must now take up one of the strangest tales in the history of astronomy—the tale of the " white dwarf." In 1844 Bessel wrote to John Herschel from Königsberg pointing out a periodic irregularity in the proper motion of Sirius. In its slow motion in the heavens it seemed now to hurry forwards, now to progress more slowly, and to repeat this cycle every fifty years. He suggested that Sirius was travelling through space with a large but dark companion, with which round their common centre of gravity it revolved in a period of fifty years. Eighteen years later this faint companion of Sirius was detected by Alvan Clark while testing one of his new lenses. The distance of Sirius is now known with considerable accuracy, and the long series of observations of the motion of it and its companion round their common centre of gravity has led, in the manner described earlier, to an accurate knowledge of the weight of each of them. It turns out that although there is an enormous difference in the brightness of the two stars their weights are not very different. Sirius is ten thousand times brighter than its companion but less than three times as heavy. Sirius is twenty-six times brighter than the sun and two and a half times as heavy. Its companion is practically the same weight as the sun, but nearly 400 times fainter. Armed however with their theory of giants and dwarfs astronomers were not particularly disturbed by these figures: there could be no doubt that the companion was a dwarf, and a low temperature star shining with a dull red glow. This explanation was never seriously questioned; the star was too faint and much too close to the dazzling Sirius for anyone to be able to see what its colour really was.

In 1915, however, Professor Adams at Mount Wilson found himself in a position to check this point. As a result of the most able technique, he managed with the 60-inch reflector to isolate its faint rays from the glare of the Dog-star and obtain a photograph of its spectrum. To his surprise the spectrum, instead of being that of an M star shining with a dull red glow, was that of an A, or hydrogen star, the surface of which must be emitting an intense white light. By Professor Hertzsprung's formula the diameter of the companion could then be calculated. The answer was that the companion had only three times the diameter of the earth, or one thirty-seventh the diameter of the sun. This gave the extraordinary result that although this star had practically the same weight as the sun it had only one fifty-thousandth of its bulk. The cramming of so much matter into so small a space implied that each cubic inch of this star must weigh on the average over a ton. One could easily push two or three tons of it into one's waistcoat pocket. Material of this sort, three thousand times denser than platinum, which is the densest substance known, seems incredible. Astronomers, in fact, *were* incredulous. Yet there appeared to be no doubt as to the observations or the deductions therefrom. But something must have gone badly wrong somewhere; and one seemed to be driven to the conclusion that there must be a blunder in one of those fundamental theories which appeared everywhere to fit so well and upon which so much else depended. One got the impression that there was a tendency for the whole matter to be rather hushed up; and certainly one heard strangely little about the companion of Sirius for the next nine years. Sir Arthur Eddington appears to have taken a different view and blamed the star itself—at least he tells us that he felt like saying, " Shut up. Don't talk nonsense." Fortunately for astronomy he took good care to inquire very carefully into the cause of the nonsense which the star—or rather its spectrum—was talking.

It was just at this time that Sir Arthur Eddington was engaged in the theoretical investigation of the internal constitution of the stars. This investigation was to lead to results of far-reaching importance in regard to the weights and the candle-powers of the stars, but since it was also to give him the key to the nonsensical message of the companion of Sirius we must break off for a moment to consider it.

It would on the face of it appear ludicrous to suppose that we could ever learn anything about the inside of a star; for the surface of a star is completely opaque and not even a spectroscope can penetrate beneath it. Here, however, mathematics comes to our assistance; and Sir Arthur Eddington with the aid of his " analytical boring machine " has obtained valuable information about the deep interior and even the extreme centre of a star.

The pressure which a gas exerts upon the walls of its container is determined by the number of impacts per second upon the walls by the rapidly moving molecules (or atoms) of the gas. The frequency of the impacts clearly depends on two things: on the number of particles, atoms or molecules, in each cubic inch, and on the speed with which they are moving to and fro. Finally, the speed at which the particles of a gas are moving is determined by its temperature, the higher the temperature the greater the speed. Briefly, then, the pressure of a gas is determined by its temperature and the number of particles per cubic inch.

In a football there are two opposite tendencies at work which exactly balance each other. The bladder as a result of its elasticity is all the time trying to contract; the pressure of the air within it is all the time trying to expand the bladder. It is only because these two just balance that the football retains a definite size. If we diminish the pressure in the bladder, either by letting some air out or by cooling it in a refrigerator, the football will shrink in size until the balance is again equalized. If we increase the pressure by blowing in more air or by warming it in front of the fire, the ball will expand until the balance is again equalized, unless of course it gives way and bursts. It follows that if we know the contracting force exerted by the bladder and also know how much air there is inside it, we can at once calculate what the temperature of the contained air will have to be to bring the ball to any required size.

To what extent can the same sort of reasoning be used for finding the temperature inside a star? Let us suppose that at a given point beneath the surface of a star we can calculate the weight of the column of overlying gas between that point and the surface which will be pressing down upon the gas at that level. Since the star is in equilibrium neither expanding nor contracting—for the majority of stars remain sensibly unaltered for hundreds of years—we know that the weight of the overlying gases must be exactly balanced by the pressure of the gas at this level. But here a difficulty arises. If we could be sure that the number of gas particles (atoms in this case) per cubic inch are the same everywhere all through the star, then we could work out what the temperature would have to be to give a pressure which would exactly balance the weight. But, as everyone knows, a gas is compressible; consequently as we go deeper into a star the weight of the overlying gases becomes greater, while the gases beneath become more and more compressed and thus contain more and more atoms per cubic inch. It is therefore necessary to investigate the rate at which the compression of the gases increases as we pass inwards from the surface towards the centre of the star.

Now this actually is a highly complicated mathematical problem which is still far from being completely solved. However, by assuming that the compression increases inwards according to a certain definite rule it is possible to work out what the temperatures will be at different depths. Fortunately it is found that if one adopts the different possible rules giving the rate at which the compression might increase, there is not a great deal of difference in the various central temperatures obtained. The reasons for this must be taken for granted.

Suppose we have a star of known size and known weight, and suppose also that we know the rate at which the compression of the gases increases towards the centre. We can then calculate for a given depth the total weight of the overlying gases; and since this weight must be exactly balanced by the pressure of the gases at that level, it follows that we know also what this pressure is. The next step therefore is to calculate what temperature is necessary to produce this pressure. This brings us to our second difficulty. The pressure depends, as we saw, on the number of atoms per cubic inch of gas and on the temperature. We know what the pressure is, and so in order to get the temperature we must also know the number of atoms per cubic inch of gas. We have so far rather glossed over how this is to be discovered. We know the total weight of the star and we know its size, and so dividing one by the other we get the average weight of each cubic inch of it. Further, since we know the rule giving the rate at which the density increases inwards, we can also get the actual weight of a cubic inch of the gas at the level under consideration. If the star was composed of pure hydrogen or of pure lead or of any other single element we could at once work out how many atoms there were in this cubic inch of gas. But the result would be very different according to whether it was a lead star or a hydrogen star. For since the atom of lead is 207 times as heavy as the atom of hydrogen, the cubic inch would contain 207 times as many atoms if it were a hydrogen star as it would if it were a lead star. In other words, the temperature required to give the same pressure in the two stars would be enormously higher in the lead star than it would be in the hydrogen star. Clearly, then, the calculated temperature would depend very much on the chemical composition of the star.

Now spectroscopic observation gives an approximate idea of the relative proportions of the different elements in a star's atmosphere. It was hardly fair, however, to assume that the elements were present in those same proportions in the middle of a star. In fact it seemed reasonable to suppose that the heavier elements would gravitate towards the centre. The question was an extremely difficult one to decide, but finally Sir Arthur Eddington determined to take the figure fifty-four as representing the most probable value for the mean atomic weight of all the atoms inside the sun and stars, and calculated the internal temperatures on this assumption.

But in 1917 the same investigator arrived at a most important conclusion which modified very drastically his previous decision and put the whole question on a much more certain footing. There was no doubt at all that the temperature at the centre of a star was at least several millions of degrees centigrade. Recent work on the ionization of atoms suggested that at such enormous temperatures as these the electron-stripping process would be carried almost if not entirely to its completion. The gases would then consist of bare atomic nuclei and free electrons. Now what will be the effect? We saw that if we had a certain weight of unionized hydrogen occupying a space of a cubic inch, and the same weight of vaporized and unionized lead occupying a similar volume, the cubic inch of hydrogen would contain 207 times as many particles as the cubic inch of lead. If, however, both gases were completely ionized, then the number of particles in the cubic inch of hydrogen would be doubled—for each hydrogen atom would break up into its nucleus and an electron; but the number of particles in the cubic inch of lead would be increased eighty-three times—for each lead atom would break up into its nucleus *plus* its eighty-two revolving electrons. Thus whereas in the case of the unionized gases the cubic inch of hydrogen contained 207 times as many particles as the lead, in the case of complete ionization it will contain only five times as many particles.

It is clear, therefore, that if the atoms in the interior of a star are completely ionized the factor of doubt as to the number of particles per cubic inch is enormously reduced. The cases we have quoted are extreme ones and yet even then the doubtful factor is only five. Upon any reasonable assumption as to the chemical composition of a star the doubtful factor will be still smaller.

To make this point quite clear let us suppose we have a number of small vessels each with a volume of one cubic inch; and let us put into each one of these respectively equal weights of all the various elements: hydrogen into the first, helium into the second, and so on up to uranium which goes into the last. If each of these elements is vaporized, but not ionized, the number of particles in each vessel will be very different. For example, the hydrogen vessel will contain four times as many particles as the helium one, sixteen times as many as the oxygen one, 207 times as many as the lead one, and 238 times as many as the uranium one.

If now the elements in the vessels are completely ionized, the number of particles in the different vessels will become very nearly the same in each case. In fact, with the sole exception of the two vessels containing the hydrogen and the helium the number of particles in each vessel will be almost exactly the same; the helium vessel, however, will contain nearly twice as many as any of the others, and the hydrogen vessel about four times as many as the others, except of course the helium vessel.

From this it follows that the calculation of the temperature at a certain depth in a star will now be practically independent of its chemical composition, unless it happens to contain a disproportionately high percentage of either helium or hydrogen. Yet even in a star composed entirely of hydrogen—an extreme case—the central temperature would only be three times as high as in a star which did not contain a disproportionate amount of this element.

We must now refer to the important results which Sir Arthur Eddington arrived at in regard to the effects in the interior of a star of light pressure or more correctly, " radiation pressure." These results were actually obtained before those which we have just discussed in regard to ionization; but for the sake of simplicity it is easier to deal with them in the reverse order. It had long been known from laboratory experiments that a beam of light is capable of exerting a very small mechanical pressure. In 1916, in the first of his series of papers, Sir Arthur Eddington showed that under the conditions existing in the interior of a star radiation pressure becomes enormous and is able to play an appreciable part in supporting the weight of the overlying gases. He found that the part it played increased rapidly as the weight of a star increased. In the dwarf stars the fraction of the weight of the overlying gases which it is able to support is practically negligible; but in the giant stars which are about a hundred times heavier it is capable of supporting practically all the weight. This has two important results. In the first place it will affect very considerably the calculations of the ordinary gas pressures and temperatures inside the heaviest stars. In the simplified theory given above—in which radiation pressure was neglected—we saw that at a given depth the weight of the overlying gases must be carried, and therefore exactly balanced, by the pressure of the gases at that level. This argument still holds good for the smaller stars and for those whose weights do not greatly exceed that of the sun. But when we come to stars more than twice as heavy as the sun, radiation pressure begins to play a significant part and begins to give considerable assistance to the ordinary gas pressure in supporting the weight of the overlying layers. Before the importance of radiation pressure in these most massive stars was realized, it was necessary to assume impossibly high central temperatures in order to give a sufficient gas pressure to support the stupendous weight of the overlying layers of the star. But now that it had been shown that a large part—and in the heaviest stars the greater part—of the weight was supported by radiation pressure, the central temperature came out very much lower than before, in fact considerably lower than in the dwarf stars.

It is now believed that the sun and stars smaller than the sun have central temperatures of about twenty million degrees centigrade, and that the more massive stars have central temperatures ranging between this temperature and a temperature of about one million degrees centigrade.

The other interesting consequence of the effect of radiation pressure pointed out by Sir Arthur Eddington was its bearing upon the extraordinary uniformity in the weights of the stars. We have several times emphasized the fact that, although the stars differ from one another enormously in size, density and candle-power, there is remarkably little difference in their weights. The great majority of stars have weights ranging between one-third of the weight and ten times the weight of the sun; while no star is definitely known which has as little as one-tenth of the sun's weight or as much as a hundred times the sun's weight. It is easy enough to see why there should be a lower limit to the weight, because a star smaller than this would be too faint to be seen unless it was extraordinarily close to us. But why should there be any upper limit? The reason is that in stars with much more than ten times the weight of the sun the radiation pressure comes very near to neutralizing gravitation. Since a star is held together by the gravitational forces acting between its component parts we should expect any abnormally massive star, even if it occurred, to break up on the least provocation.

The last step in this investigation was to discover the way in which the conditions in the interior of a star would determine the candle-power with which it shone. The candle-power of a star is nothing more than the amount of light which is each second pouring out from its surface. The source of the star's light—or more correctly of its radiation, for the *visible* radiation is but a small fraction of the total radiation—is in the star's interior. Just as water always flows down hill at a rate which is determined by the slope, or the gradient, of the hill, so radiation flows in the direction of falling temperature at a rate which depends on the " temperature gradient." Other things being equal, the rate at which the radiation flows outwards from the star's intensely hot centre towards its cooler surface will be greater the higher the central temperature. But the passage of the radiation from the centre to the surface of a star is a very rough one. Countless numbers of times every second each little parcel of radiation, as we may picture it, is absorbed by the myriads of atoms thronging the body of the star; and after each absorption it is emitted again in quite a different direction. So it is only after a highly zigzag and very much drawn-out trip that any particular parcel of radiation finally reaches the star's surface and starts at last on its unimpeded journey through space. This opposition set up by the atoms in the star to the outward flow of radiation represents actually the *opacity* of the star. If it did not exist, if the star was perfectly clear and transparent, then the escape of the radiation at the surface would be enormously more rapid than it is, and the stars would shine very much more brightly than they do.

We thus see that there are two processes which are working in opposition to one another. On the one hand there is the flow of radiation from the intensely hot centre towards the cooler surface, which is determined by the central temperature and the temperature gradient of the star. On the other hand this outward flow is being continually frustrated by the capacity of the atoms to absorb the radiation, or what comes to the same thing, by the opacity of the star. The observed brightness of a star will finally depend on the rate at which the radiation, after fighting its way through the star, leaks out at its surface. We have seen how Sir Arthur Eddington's theory had already dealt with the first of these two processes. It remained therefore to investigate the process of absorption and find the opacity of a star. This was a problem of extreme difficulty and it was not until 1923 that it was successfully worked out by Dr. H. A. Kramers. The theory of both processes was now available and in January 1924 Sir Arthur Eddington was able to calculate the brightness of an actual star from the theory and compare the result with observation. He selected the bright star Capella, which happens to be a giant double star for which the necessary data are known with considerable accuracy. But unfortunately the result which the theory gave was nearly ten times what it ought to have been.

Sir Arthur Eddington, however, was not put off by this apparent failure of his theory. It seemed to him that that part of it which had to do with the calculation of the opacity of a star was less convincing than the rest of the theory. He therefore went ahead and used the theory to calculate the brightness of all the other stars for which the necessary data were available just as he had done in the case of Capella. The results of this investigation were presented to the Royal Astronomical Society at the meeting in March 1924 in what is undoubtedly one of the most important astronomical papers of recent years.

The candle-power of a star as we have seen must depend on two factors: the tendency of the radiation to flow outwards from the centre, and the opacity which tends to hinder that flow. A knowledge of the total weight of a star and of its size enables one to calculate its average density or the average weight of each cubic inch of it. From the theory as to the rate at which the density increases towards the centre one can then work out the actual densities at different depths and hence the weights of the overlying gases at these depths. This tells us the pressure necessary to support this weight. Finally, after allowing for the effects of radiation pressure and for the completely ionized state of the atoms one can work out at what temperature the gases at this level will have to be in order to exert that particular pressure. Hence we obtain the central temperature of the star and its temperature gradient, or the rate at which the temperature drops from its centre to its surface. And this, as we have said, gives the rate at which radiation, if unobstructed by opacity, would flow towards the surface. The opacity upon the other side of the scale can be shown to depend on the density of the gas and on its temperature and also upon a certain constant the value of which was still doubtful. Sir Arthur Eddington had shown that out of all this a quite simple relation could be deduced between the candle-power of the star on the one hand and its weight, its radius and this doubtful opacity constant on the other.

When the doubtful opacity constant was accepted, the relation gave, in the case of Capella, a brightness about ten times too great. And now when he made use of the same relation in calculating the candle-powers of all the other stars for which the data were available, they all with one exception came out too bright by exactly the same amount. But if on the other hand he admitted the uncertainty of the opacity constant and simply used Capella for determining its value and then inserted the value thus determined into the *candle-power-weight-radius relation*, he then got for all the other stars precisely the correct brightness. Now this constituted a very powerful argument in favour of the essential correctness of the theory, for forty-five out of the forty-six stars of which the candle-powers had been so successfully calculated by it extended over more than a millionfold range in candle-power. In comparison with this achievement the failure of the theory to give the correct constant—and then by only a factor of ten—sank into insignificance, especially when one considers the great complexity of the investigation. Although the failure in regard to the constant was at first attributed to the uncertainties in the physical theory of opacity, Sir Arthur Eddington subsequently suggested an alternative explanation. We saw that as soon as he realized that the atoms in the interior of a star must be more or less completely stripped of their electrons, the theory became automatically independent, or nearly so, of the chemical composition of the stars. It made no difference to the conditions in the interior whether a star were made of gold, lead, iron or any other substance. But there was one proviso, and that was that it must not contain an excessive preponderance of the element hydrogen. If, however, this *were* the case and the stars did after all contain a high percentage of hydrogen, then, as Sir Arthur Eddington showed, the discrepancy of the factor of ten between the observed and the calculated constant might completely disappear. At the time this large excess of hydrogen seemed unlikely and he preferred to put the blame on the doubtful opacity constant. Since 1924 the pendulum has swung in the opposite direction; Dr. Kramers' theory of the absorption of radiation has been substantiated, and it seems that the theoretical value of the opacity constant cannot be far wrong. Instead, it is generally believed that Sir Arthur Eddington's so-called " mass-luminosity law " leads to the conclusion that the stars with remarkable constancy contain as much as thirty per cent. of hydrogen. But why there should be this tremendous preponderance of hydrogen over all the other elements is quite unknown.

The reason why the relation between the luminosity or candle-power on the one hand and the radius and the weight or mass on the other is called the " mass-luminosity law " is that according to it the luminosity of a star depends very much more upon its mass than upon its radius. For example, if the mass is increased fourfold the luminosity increases thirty-twofold; whereas, if the radius is increased fourfold the luminosity increases only twofold. It is only in a very few stars that the radius can be measured directly—a few eclipsing double stars and the few stars within reach of the interferometer. But we saw when considering Professor Hertzsprung's work on the theoretical determination of stellar diameters that there was a definite relation between the diameter of a star, its luminosity and its surface temperature or colour. Hence it was possible for Sir Arthur Eddington to express his mass-luminosity law in an alternative form, and to replace his relation between luminosity, mass and *diameter* by a relation between the luminosity, mass and *surface temperature*. Expressed in this way the law enabled one to calculate the weight of any star of which the candle-power was known from measurements of its apparent brightness and distance, and of which the surface temperature was known from a study of its spectrum or of its colour. The mass-luminosity law thus provided astronomers with a powerful method for increasing their knowledge of stellar weights, for previously a star could only be weighed if it happened to be a certain type of double star.

It is interesting to realize that all these intricate investigations into the conditions existing inside a star and the deductions to be derived from them in regard to the weights, sizes, luminosities, and colours of these bodies, were carried out independently of astronomical data. They depended entirely on data derived from laboratory experiments and on the application of general physical principles also derived from laboratory experiments. If Sir Arthur Eddington had had the misfortune to have been born on Venus, whose atmosphere of dense cloud must for ever obscure the starry heavens, it is comforting to think that he could still have worked out what, if they existed at all, the stars must have been like. He could have foretold that if there were bodies which were self-luminous they must have weights ranging between 200 billion, billion and 200 thousand, billion, billion tons, that is between one-tenth and one hundred times the sun's weight. And he could have foretold the way in which their luminosities and their colours depended on their weights and sizes. There is no better example of the great power of mathematics and physics, and it illustrates well the relationship which in recent years has been growing up so closely between the microscopic physics of the atom and the macroscopic physics of the stars.

But let us return to the memorable meeting of the Royal Astronomical Society in March 1924. Having shown on the screen the theoretical curve drawn from the mass-luminosity law with the forty-five stars lying beautifully upon it, Sir Arthur Eddington then horrified his audience by informing them that unfortunately the great majority of these stars were the wrong ones. His theory applied only to gaseous stars and therefore presumably only to the rarefied giant stars. Yet the majority of the stars on the curve were dwarf stars of considerably high density.

In the theory of the internal constitution of the stars the theory of what is known as a " perfect gas " plays a very essential part. A perfect gas is a gas which obeys the well-known law of Robert Boyle, who has been aptly described as " the father of physics and the uncle of the Earl of Cork." Boyle's law states that if the pressure to which a gas is subjected is doubled, the volume which it occupies will be halved. If we imagine a quantity of gas contained in a vertical cylinder which is closed in at the top by a freely moving piston, then the more weight we place upon the piston the lower will it sink into the cylinder and the smaller will be the volume into which the gas is compressed: doubling the weight halves the volume, increasing the weight fourfold divides the volume by four, and so on. But sooner or later there comes a point, when the pressure is very high, at which the law begins to fail, and a further doubling of the weight will no longer quite halve the volume. The reason is that the law only holds good so long as the gaps between the molecules or atoms are large in comparison with the sizes of the individual molecules or atoms. But when the compression is so great that the intervening gaps are practically " squeezed out," the individual atoms or molecules begin to jam against one another and the law breaks down. The gas then ceases to behave as a " perfect gas." Now it had always been supposed that the essential difference between giant and dwarf stars was that the giant behaved as a perfect gas while the dwarf did not. A cool rarefied giant contracted under its own weight in accordance with the gas law, and in so doing its temperature gradually rose. So it passed through the various giant stages, through orange and yellow heat to white heat, becoming at the same time smaller and denser. On arriving at the white-hot stage the density reached a point at which the atoms began to jam and the gas law to fail. Thereafter contraction would be slowed down, and at the same time the rate at which heat would be produced would also diminish. Up to this point the production of heat in the interior had been more rapid than the escape of heat from the surface with the result that the temperature of the star had been increasing. But now the rate at which heat was being produced would fall below the rate at which it was escaping at the surface and the star's temperature would necessarily begin to fall. The star would therefore begin to retrace its steps along the colour series and pass down the " main sequence " towards the red dwarf stage. Thus, on the then accepted theory of stellar evolution, due mainly to the work of Professor Russell, the point at which a star reached its maximum temperature and passed over from the giant stage into the main sequence, was the point at which the density of the stellar gases became high enough for the atoms to jam and the perfect gas laws to fail.

How then could a theory based essentially on the laws of a perfect gas fit in so beautifully with those stars which were essentially supposed not to behave as a perfect gas? It looked for a moment as if the whole theory of the internal constitution of the stars must be at fault. Rather like a conjuror who is always able to produce some new surprise from his sleeve, so Sir Arthur Eddington, immediately after pointing out that they were apparently the wrong stars, proceeded to show that in spite of all previous theories there was nothing wrong with them at all. He showed that the answer to the problem was to be found in that paper of his eight years before in which he had come to the conclusion that the atoms inside a star are completely stripped of their electrons. The size of an individual atomic nucleus or of a free electron is minute in comparison with that of an intact atom, just as the individual sizes of the sun and planets is minute in comparison with the size of the whole solar system. In fact an atomic nucleus or a free electron has but one hundred-thousandth part of the bulk of an average atom. And so it would be only reasonable to suppose that a gas with completely stripped atoms could be compressed many thousand thousand times as much as an ordinary gas before its atoms began to jam and before the gas laws began to fail. Hence there was no reason at all why the dwarf stars should not fit into the curve of the mass-luminosity law.

This remarkable result necessarily led to a complete reconsideration of the accepted theory of stellar evolution and of the significance of the two sequences of colours, spectra and surface temperatures represented by giants and dwarfs. But before considering these matters we must take up again the tale of the White Dwarf at the point at which we left it. For after an interval of nine years the nonsensical message of the companion of Sirius had been solved and the key to the mystery had suddenly materialized out of the mass-luminosity law.

That the mass-luminosity law was obeyed by the wrong stars, the red dwarfs, meant that the gases composing them still behaved as perfect gases in spite of their densities being comparable with that of water. And this, we saw, could only mean that the atoms inside such stars were largely stripped of their electrons and could thus be subjected to much higher degrees of compression than ordinary atoms without beginning to jam. It followed that the density at which atomic jamming became complete and further compression impossible would also be much greater than in the case of ordinary atoms. We saw that if the electrons were completely stripped from their atoms the amount of room occupied by the nuclei and the electrons would be reduced many thousands of times. And so the message from the companion of Sirius, that its average density was 3,000 times that of the densest of known terrestrial substances, was no longer to be regarded as necessarily nonsensical. Actually, of course, we should not expect so dense a star to show *no* signs of atomic jamming; and in point of fact the companion of Sirius was the one star which Sir Arthur Eddington had found to disobey his law.

Although the excessively high density which observation had appeared to demand for the companion of Sirius no longer contradicted the tenets of common sense, it was obviously desirable that it should be confirmed if possible by some quite independent observational method. Now the man who had been in the first instance the cause of all the trouble—the man who had, so to speak, taken down the extraordinary message and published it—was Professor Adams. And so it was most appropriate that he should now be the man who, in a most ingenious manner, obtained an independent check on the message.

According to Einstein's theory of Relativity all periodic processes taking place in the neighbourhood of a massive body must be slowed up. This must apply to the light vibrations emitted by atoms. For example the vibrations corresponding to the particular electron-jump in the hydrogen atom responsible for the red hydrogen-line will be slightly slower if the atom is situated near the surface of the massive sun than they would be if the atom was in a laboratory on the much less massive earth. A slower vibration means a smaller frequency and a greater wave-length, and that, as we have already seen, will imply that the spectral line will be shifted slightly towards the red end of the spectrum. In other words the hydrogen-lines in the solar spectrum which are due to atoms in the sun's atmosphere will all be displaced slightly towards the red end of the spectrum as compared with the same lines obtained from an electrical discharge through a tube of hydrogen in the laboratory. Einstein showed that the displacement of the lines would on the one hand be proportional to the weight of the heavy body affecting the atom, and on the other inversely proportional to the distance of the atom from the centre of the body. Now if the message from the companion of Sirius was correct it meant that though this star was about equal to the sun in weight, its radius was only about one-thirtieth of the sun's radius. Thus the distance separating the atoms in the atmosphere of this star from its centre, was thirty times less than the distance separating the atoms in the solar atmosphere from the centre of the sun. And since the weight of the two bodies was the same, the displacement of the spectral lines of the companion of Sirius should be just thirty times as much as the displacement of the same lines in the sun. The measurement of the Einstein displacement in the spectral lines of a star gives one therefore the ratio between the weight and the radius of the star.

As to the weight of the companion of Sirius there could be no shadow of doubt, for it is one of the most accurately weighed members of a double-star system in the heavens. If now one could get in addition the ratio between the mass and the radius from the Einstein displacement in the spectrum, one would have in effect obtained an almost direct determination of its radius, and hence also of its density. The previous determination of the radius made by Professor Adams, which had been the cause of all the trouble, had been got, *via* Professor Hertzsprung's formula, from the surface temperature of the star derived from a study of its spectrum. Although it seemed very unlikely that there could be any fallacy in this determination of radius from the temperature, it was just conceivable that in a star of such exceptional character the spectral appearances might be misleading. But this was not the only reason why Professor Adams now set about the difficult task of measuring the Einstein displacement in the companion of Sirius; for if he succeeded he would have killed no less than three birds with one stone. He would have confirmed beyond doubt the nonsensical message, he would have provided strong observational support for one of the fundamental assumptions in the theory of the internal constitution of the stars, and he would have given in addition important experimental confirmation of the relativity theory.

But the task he had set himself was excessively difficult. The companion is so faint and so near to the dazzling Sirius that it is almost impossible to isolate the rays of the fainter star sufficiently for spectroscopic study. It had been a magnificent achievement when in 1915 he had obtained a spectrum good enough for a temperature determination. Yet for the purpose now in hand a very much better spectrum would be necessary. With the 60-inch telescope at Mount Wilson which he had used in 1915 it would not have been possible; but with the 100-inch which had since been built there was just a chance. So in the winter months 1924–25 he made the attempt and succeeded, and was able to publish in the following summer the results of the measurements.

They confirmed exactly the deductions from the temperature determination of 1915: the nonsensical message was sensible after all, and each cubic inch of this star did on the average weigh one ton.

There are now about half a dozen stars known definitely which are similar to the companion of Sirius. Essentially they are stars of extremely low luminosity or candle-power in spite of their surfaces shining at an intense white heat. It is for this reason that they are classed as the *white dwarfs* to distinguish them from the ordinary type of dwarf star which is yellow, orange or, most typically, red in colour. Their rarity must be apparent rather than real. Their very low luminosity means that even the closest of them will not be bright enough to attract attention. Consequently they will continue to be discovered only by accident until it is possible to extend routine studies of stellar distances and spectral types to great numbers of very faint stars. The fact that no less than three of the white dwarfs are among the two dozen nearest stars makes it extremely probable that they are in reality very abundantly scattered through space.

A star will at once be suspected of being a white dwarf if in spite of being very faint it is found to be very close to us and to have a white colour. An accurate determination of its distance, its apparent brightness and its surface temperature will then enable its diameter to be calculated and a definite decision made as to whether or not it is a white dwarf. However, it will only be possible to give an approximate estimation of its density unless we can also measure its weight. This cannot be done except when the star is a member of a double-star system. We cannot calculate its weight from its luminosity *via* the mass-luminosity law, because the white dwarf is the one type of star to which that law does not apply. Nor does the Einstein displacement in the spectrum give us any help. To determine that displacement we must first make allowance for any displacement of the lines due to the motion of the star towards or away from the earth. It is only when the star is one of the components of a double star that we can disentangle the two kinds of displacement by measuring also the spectrum of the other component. Only one other white dwarf has had its density accurately measured and that turns out to be even denser than the companion of Sirius. Among the remainder, of which the densities can only be approximately estimated, there are one or two stars that appear to have a still greater density. The most remarkable is a star which was studied by Dr. G. P. Kuiper of the Harvard Observatory in 1935. The radius of this star is about half that of the earth, yet it is believed to weigh nearly three times as much as the sun. Its average density is probably about 600 times that of the companion of Sirius, so that each cubic inch of it weighs on the average about 600 tons. Another interesting thing about this star is its extremely high temperature. We saw that in the hottest O type stars the spectral lines were faint and not numerous, and that theoretically, at a somewhat hotter stage, the atoms in the star's atmosphere would be so highly ionized that all the spectral lines would be out of reach in the invisible ultra-violet part of the spectrum. It seems that Dr. Kuiper's star is hot enough to fulfil these conditions: at least no photograph yet taken of its spectrum has succeeded in revealing any spectral lines.

There are many questions which we should like to ask about the white dwarfs: What stage do they represent in the march of stellar evolution? What is the mechanism by which they are produced? And finally, is their formation a normal or an abnormal event in the life-history of a star? For the moment we must postpone the discussion of these problems.

CHAPTER XIII

THE SOURCE OF STELLAR ENERGY AND THE AGE OF THE UNIVERSE

It was not surprising that Sir Arthur Eddington's theory of the internal constitution of the stars was received with acclamation. It put all the stars for which the data were available so beautifully into their proper places, it explained why the stars had the weights which they actually did have, and it solved the mystery of the incredible densities of the white dwarfs. Yet the success which it achieved in these directions was for a moment overshadowed by the havoc which it wrought among the generally accepted views on stellar evolution.

It follows from the mass-luminosity law that a star of any specified weight must have an absolute luminosity within certain very narrow limits. It is true that the luminosity of a star depends to a minor degree upon its radius, and that of two stars of the same weight the smaller of the two will be slightly the brighter. It might have been supposed that the smaller star would be the fainter in proportion to its smaller surface area. But provided its weight is the same the smaller star will be denser and therefore hotter; its surface will thus shine with a whiter and more intense light, and this will more than compensate for its smaller surface. Yet even in an extreme case the smaller white star cannot be very much brighter than a large red star of the same weight. And certainly the difference is negligible in comparison with the enormous differences actually found among the stars in the sky. The mass-luminosity law makes it quite clear that if two stars differ considerably in luminosity then the brighter one must be the heavier. Of course, as we have frequently emphasized, the luminosities of the stars extend over an immensely wider range than do the weights of the stars. But that is because a small change in weight carries with it a large change in luminosity—in fact a twofold increase in weight implies more than a tenfold increase in luminosity. In other words, the weight, though not varying very much from star to star, is nevertheless the main factor in determining the luminosity.

But how was this to be reconciled with the Russell-Hertzsprung giant and dwarf theory which attributed to the spectral sequence an evolutionary significance? On that theory a star was supposed to start life as a red giant, pass through a stage of contraction and rising temperature to a white giant, and then turning the corner of the sequence gradually cool down to end its life as a red dwarf. In it the determining factor, the driving force which so to speak urged it first up and then down the spectral sequence, was its process of contraction. Yet now according to the mass-luminosity law a star could only pass through these stages by actually losing weight. The idea that a star lost weight as it grew older was in direct contradiction to what was perhaps the most fundamental principle in physics. The law of the conservation of mass stated that the weight of all the matter in the universe could never increase or diminish by one iota. Under certain exceptional circumstances a star might break up into two or more fragments or it might eject material from its surface; but these were rare and critical events in the life of a star and certainly could not account for the observed spectral sequence. No, the mass-luminosity law left open only two possible courses: either one must reject the evolutionary significance of the spectral sequence or one must throw over the law of the conservation of mass.

If we adopt the less revolutionary view and assume that the weight of a star does not alter during its lifetime it is impossible to retain the Russell-Hertzsprung scheme of evolution. The mass-luminosity law insists that a very heavy star must have a correspondingly high luminosity, but permits it to have a very large range of size, density and colour. Up to a point this will fit in with the facts; for heavy stars, the members of the giant sequence, while being all very much of the same candle-power may be extremely large, red and diffuse, or comparatively small, comparatively dense and white. Thus the giant sequence could still be regarded as an evolutionary sequence: the red-hot giant gradually contracting down to become the white-hot giant. But among the stars of the main sequence, the yellow stars and the red dwarfs, which have comparatively small weights and correspondingly low luminosities, the range of colour and density is very narrow. Thus we know of no red dwarfs that are in a rarefied condition and from which the ordinary dense red dwarf could have developed. So long as it was assumed that the weight of a star did not change, it was thus impossible to attribute any evolutionary significance to the main sequence; instead the main sequence would have to be regarded simply as indicating the particular density, size and colour which a star of a given initial weight would tend to assume. For example it might be supposed that the various combinations of weight and luminosity on the one hand with size, density and colour on the other, which were not found represented among the stars, corresponded to evolutionary stages through which the stars passed so rapidly that it was improbable that we should come across them.

But what about the alternative possibility? Could anything be said in favour of the highly revolutionary step of discarding the law of the conservation of mass? A quarter of a century earlier the proposer of such a thesis would have found himself classed in the same category as the advocates of perpetual motion. But in the meantime many curious things had happened in science and the possibility that the law might have to be modified was no longer to be dismissed with laughter. It had long been a major problem in astronomy to account for the *source* of solar and stellar radiation. How could the sun and stars go on radiating heat and light for so long without exhausting their supplies? In 1853 Helmholtz had propounded his famous "contraction-hypothesis." According to this the sun was continually reinforcing its supply of heat by slowly contracting in size, its gases tending to fall inwards under their own weight towards its centre. Just as a falling stone has its energy of motion converted into heat when it is suddenly brought to a standstill by the ground, so the energy of motion of the solar gases as they slowly collapse inwards towards the sun's centre is continually being converted into the heat and light which the sun supplies to us. But when in 1862 Lord Kelvin came to work the theory out in detail he found that even if the sun had started with an almost infinite size the amount of heat it would have evolved in contracting to its present dimensions could not have kept it going, even at its present rate of radiation, for more than 50 million years. But from geological evidence alone we know that it must have been shining for a far longer period than that. It was then suggested that the sun might be particularly rich in radio-active material, and that the source of solar radiation came in part from the sub-atomic energy liberated as these unstable atoms spontaneously broke down into simpler atoms. But still the supply of energy was hopelessly inadequate.

In 1904 an entirely novel suggestion was put forward by Sir James Jeans. He suggested that under the extraordinary conditions existing inside a star matter might be actually converted into radiation by the mutual annihilation of a proton and an electron. In the following year Einstein's Special Theory of Relativity was published, in which matter and energy were identified as two forms of the same entity. The theory also provided the means for calculating how much energy would be produced if a given quantity of matter was completely converted into energy. This enabled Sir James Jeans to calculate how long the conversion of matter into radiation would maintain an adequate supply of heat and light for the sun and stars. Oddly enough he found that the supply was now apparently very very much bigger than was necessary. If the sun and stars were able to convert their substance completely into radiation he showed that they would go on shining for several millions of millions of years.

In the next twenty years this remarkable theory gained greatly in plausibility. During this period the Generalized Theory of Relativity had grown out of the Special Theory of Relativity and had won general acceptance as a result of its amazing achievements. There could no longer be any doubt that matter and energy were a single entity, although it did not necessarily follow that the one was ever transformed into the other. Again, certain investigations had shown that the ages of the stars must be very much greater than had previously been supposed. We saw that in 1911 Dr. Halm had pointed out that the heavier stars were moving more slowly than the lighter ones; so that if the stars were grouped in classes according to their weights the average energy of motion (the weight of the star multiplied by the square of its velocity) would be the same for each class. He suggested this was an example of the theorem of the " equipartition of energy " which is known to apply to a mixture of gases. As a result of their frequent collisions the molecules of the heavier gas are slowed down and those of the lighter are speeded up until after a very brief interval the average energy of motion of the light and heavy molecules is the same. The same thing ought to come about among the stars after the lapse of a long enough interval. Collisions between stars will occur too rarely to have any effect, but periodically a star will approach close enough to another star for their mutual gravitational attraction to be appreciable. The repetition of these approaches throughout the ages will ultimately result in the equipartition of energy. From a knowledge of the average number of stars in a given volume of space the time required for this to happen can be calculated. Dr. Halm's original work was greatly extended in 1922 by Dr. Seares at Mount Wilson, who from an examination of the weights and velocities of a large number of stars of all spectral classes found a close approach to equipartition of energy except in the case of the hottest B stars. Sir James Jeans calculated that for this state of affairs to come about between five and ten million, million years would be required.

Two somewhat similar investigations were also carried out by Sir James Jeans and led to much the same result. In the first, which dated back to 1913, he studied the " kinetic theory " of star-clusters. In other words, he regarded the stars in a cluster as analogous to the molecules in a gas and applied to them the " kinetic theory " of gases. We have seen that there are certain so-called " moving clusters," in which the stars are moving with equal velocities in straight lines parallel to one another. In its progress through space such a cluster will be continually encountering the stars which happen to lie in its path; and at each encounter the stars in the cluster will tend to be deflected slightly from their previous tracks. But the lighter stars in the cluster will be more readily deflected than the heavier stars. Thus as time goes on the cluster will tend to lose more and more of its smaller members. Observation suggests strongly that in the moving clusters only the comparatively heavy stars remain, and so it is possible to calculate how long they must have been in existence for the smaller stars to have fallen out.

The other investigation was concerned with the shapes of the orbits of double stars. If an outside star happens to approach comparatively close to a double star it will tend to change the shape of the orbit in which the smaller star revolves round the larger. It may make it more elongated, that is, increase its eccentricity, or it may make it more nearly circular, according to the circumstances. Sir James Jeans showed that if such close approaches were repeated over a long enough period of time a state would be reached when certain degrees of eccentricity of the orbit would be more probable than others, and that if a large number of orbits were taken at random there would be a certain definite percentage having one degree of eccentricity, another definite percentage with another degree, and so on. The time required to achieve this particular distribution of eccentricities would as before depend on the number of stars in a given volume of space, and it would also depend on the distance between the two components of the double star. Two stars revolving very near to each other would be held together by a very powerful gravitational attraction, and it would require a great many approaches by an outside star to change the shape of the orbit appreciably. A pair of stars separated by a large distance and held together by a feeble gravitational pull would be much more susceptible to outside influences. As one would expect, the actual separation in space is very much greater in the visual double stars than it is in the spectroscopic double stars. He was thus able to show that whereas the final distribution of orbital eccentricities would be achieved in many millions of millions of years by the visual doubles, it would not be achieved for many hundred million, million years by the spectroscopic doubles. The measurements of the eccentricities of double-star orbits collected by Professor Aitken at the Lick Observatory fit in extraordinarily well with this theory: the spectroscopic doubles show no tendency whatever to approach the final distribution, while the visual doubles show a very definite approach to it. Sir James Jeans finds that these three investigations based on the energy of motion of stars of different weight, on the breaking-up of moving clusters, and on the eccentricities of double star orbits, all point to a past history of the universe extending over a period of the order of five to ten million, million years. But such a terrific time-scale was only to be obtained in one way, and that was by accepting the suggestion that matter could be annihilated, or more correctly converted into radiation. Only in this way could one account for the enormous supplies of energy necessary to keep the stars going for so long.

And so when the thunderbolt, in the form of the mass-luminosity law, fell upon astronomy there were already quite reasonable grounds for supposing that the evolutionary significance of the spectral sequence might still be saved; and that by modifying our views on the conservation of mass we might continue to regard a star as pursuing its evolutionary course through the giant and main sequences by the simple process of radiating away its weight in the form of light and heat.

If matter was to be capable of transformation into energy it was necessary to suppose that there were certain conditions controlling that transformation. It was quite obvious for example that the material inside the earth was not undergoing transformation, at least to any appreciable extent; otherwise its surface would be heated to a high temperature as a result of the liberated energy. It was therefore assumed that the annihilation of matter only began to proceed at an appreciable rate when the centre of a star reached a temperature of the order of twenty million degrees. On these lines Professor Russell in 1925 worked out a theory of stellar evolution. Roughly, his theory was as follows. As before, a massive star was supposed to start life as an enormously large and rarefied giant. The central temperature of such a star is as we saw comparatively low—one or two million degrees; within it therefore the annihilation of matter is negligible, and the star will be dependent on its contraction for balancing the radiation lost from its surface. The star will thus contract rapidly, as on the original giant-and-dwarf theory, passing in comparatively quick succession through the various giant phases of increasing surface temperature from a red to a white giant. Though the intensity of the brightness of the surface increases enormously at this time, the total brightness of the star only increases slightly owing to its rapidly diminishing size. As a result of the increasing density, the central temperature will by now have reached a level at which annihilation of matter is considerable. The radiation lost from the surface is then practically balanced by that which is being transformed out of the material at the centre; so the contraction, previously rapid, is now very markedly slowed down. The star having passed quickly through the giant stage thus embarks at this point on a vastly more leisurely march down the main sequence. Extremely slowly the star radiates away its matter in heat and light, and so, in accordance with the mass-luminosity law, very gradually falls off in brightness. The extremely slow progress made during this stage in a star's development is due to the immense amount of energy that is liberated during the destruction of matter; for a star like the sun could radiate in this way for more than one hundred thousand million years and still only use up one per cent. of its weight. During this phase the contraction of the star, though proceeding vastly more slowly than before, still continues; and it is because of the increasing *density* and increasing *opacity* of the star which accompanies this contraction, that the surface temperature of the star diminishes and its colour changes through yellow and orange to red.

The new theory differed in certain essential particulars from the old giant-and-dwarf theory. (1) In its passage down the main sequence a star continually loses weight instead of retaining a constant weight; so the red dwarf is smaller, denser and also *lighter* than it was in its giant youth. (2) The falling surface temperature during a star's passage down the main sequence is no longer the result of the failure of the gas laws; instead it is due to the increasing density and opacity of the body of the star which diminishes the rate at which the radiation makes its way from the intensely hot interior to the star's surface. Though the surface temperature falls, the central temperature changes but little throughout the main sequence.

In order to account for the white dwarfs, Professor Russell assumes that *all* the matter in a star is not necessarily available for transformation into radiation. A star may therefore exhaust its store of transformable matter while still retaining a considerable weight of non-transformable matter. If this should happen—and theoretically it might happen anywhere on the main sequence, depending on the initial poverty of the star in regard to transformable matter—the loss of radiation from the surface will again have to be balanced by the star resorting to contraction. Once more, as in the original giant stage, the star will pass through a phase of rapid contraction and rapidly increasing surface temperature; but this time there is nothing to prevent the contraction proceeding until the atoms, in spite of being completely stripped of electrons, are so closely jammed that they cannot be packed any nearer together. This is the white-dwarf stage, the final state of a star. The theory has the advantage that it explains among other things the remarkable rarity of giants in comparison with dwarfs. The giants are rare for the simple reason that a star goes through the giant stage very much more quickly than it does through the stage of the main sequence. Hence at any time, such as the present, we are unlikely to find more than a minority of stars passing through it. In the same way it explains why we do not find any stars of intermediate density between the ordinary red dwarfs and the white dwarfs—for again this particular stage is passed through with great rapidity.

The theory of the transformation of matter into energy as a result of the actual annihilation of a proton by an electron came into prominence, as we have seen, for three reasons. (1) No other theory so far considered was able to explain how the sun and stars could go on shining as they do for more than a few millions of years. (2) There was no doubt that it was the only possible theory that could give to the sun and stars a life of several million, million years such as was required according to Sir James Jeans' studies of stellar velocities, star-clusters, and double-star orbits. (3) It saved one from having to throw over the view, which was so difficult to resist and which had since the earliest days of spectroscopy received the blessing of the most eminent, that the spectral sequence possessed an evolutionary significance. But apart from astronomy was there any evidence for the annihilation of matter? Now as a matter of fact for some years past physicists had been engaged in the investigation of a mysterious form of radiation of excessively short wave-length. Observations made from balloons showed it to be much more intense at high altitudes, and thus indicated that it originated somewhere outside the earth's atmosphere. That it did not emanate from the sun was shown by the fact that it was just as strong by night as by day. There could be little doubt that it reached us indiscriminately from all directions in interstellar space. But most important of all was its extremely short wave-length, shorter than that of any other radiation known; and this, it was believed, could only be accounted for by the liberation in some way of sub-atomic energy. And so it was suggested that the "cosmic rays," as this radiation came to be called, were produced by the annihilation of a proton by an electron and came to us from those regions of interstellar space where matter was undergoing destruction. It was not supposed, however, that the cosmic rays came to us from the interiors of the stars; for any radiation of short wave-length originating inside a star will be transformed into radiation of much longer wave-length as a result of its zigzag journey through the star's opaque body. Nevertheless the existence of the cosmic rays strongly suggested that some form of transformation of matter into radiation was taking place somewhere in the universe, and thus added considerable weight to the theories we have been discussing.

There was, however, an alternative theory of the origin of cosmic rays. Professor Millikan, who more than anyone had been responsible for their investigation, favoured the view that they were the result not of the complete but of the partial transformation of matter into energy. We have seen that a helium atom though it is made up of four hydrogen atoms is slightly less heavy than four individual hydrogen atoms. The total contained energy of the consolidated helium atom is nearly one per cent. less than the total energy contained in four hydrogen atoms; and since, according to relativity, weight and energy are the same, the weight of the helium atom is also nearly one per cent. less than the weight of four hydrogen atoms. Thus if hydrogen could be transmuted into helium nearly one per cent. of the initial weight would be transformed into radiation of very short wave-length. The suggestion was that this was the origin of the cosmic rays: that somewhere in interstellar space they were being liberated in the process of the building up of helium atoms out of hydrogen atoms.

It was then suggested that the source of stellar radiation might lie in the building up of helium out of hydrogen rather than in the complete annihilation of protons by electrons. But how would such a view fit in with the facts? If *complete* annihilation occurred, then so long as the star remained in the physical condition necessary for annihilation, it could go on radiating until it had radiated itself entirely away. If on the other hand the supply of radiation came only from the building up of hydrogen into helium atoms, then even if a star consisted initially completely of hydrogen it could only go on radiating until it had radiated away about one per cent. of its weight. (We have seen that there is probably a very high percentage of hydrogen in the make-up of a star, and if this theory is correct the percentage may have been very much higher in past ages. Again, the building up of helium atoms into still more complex atoms also involves a loss of weight and liberation of energy, though much less than in the transmutation of hydrogen into helium. By taking this into account the total supply of radiation derived from the building up of heavier out of lighter elements may be regarded as approximately equivalent to one per cent. of the star's total weight.) It follows that if the source of stellar radiation is derived from the building up of heavier atoms out of hydrogen a star will only be able to go on shining for one-hundredth part of the time for which it would be able to shine if its matter were annihilated completely. Thus the adoption of the transmutation theory would imply cutting down the past history of the universe from *a few millions of millions* of years to a *few tens of thousands of millions* of years—a cut in fact of ninety-nine per cent.

This appears to be a very drastic cut in our time-scale; yet even so it left us with a time-scale which was still about ten thousand times longer than that which was available on the original contraction hypothesis. So far as geological evidence went in regard to the age of the earth it was entirely sufficient. On the other hand it was about a hundred times too short as judged by the results of Sir James Jeans as to the time required for equipartition of energy to occur in the universe. Moreover it necessitated once more the rejection of the evolutionary significance of the spectral sequence. For clearly if a star during its lifetime could only radiate away one per cent. of its weight, a star born with ten times the sun's weight could never turn into a star with one-tenth of the sun's weight.

Let us see how on this theory stellar evolution might proceed. There is good reason to suppose that the transmutation of the elements will take place more rapidly as the temperature rises. Since a star can now only change its weight by one per cent. it follows that stars must be born with weights ranging between about one-tenth and a hundred times the weight of the sun. But in any case they will start life as red stars of low density. If the star is a massive star it will contract as before through the giant sequence of ascending temperature to become a white giant. Arrived at this point the high internal temperature will accelerate the previously slow transmutation process, which will now practically balance the loss of radiation from the surface. Contraction will thus be brought almost to a standstill, and the star will continue in the white giant stage until its supply of transmutable hydrogen begins to be exhausted—a period of some ten thousand million years, during which its weight and luminosity will diminish but slightly. When this finally does happen the star will contract rapidly into the white-dwarf stage.

But what about the less luminous and less massive stars on the main sequence—the yellow stars like the sun, and the orange and red dwarfs? These stars have densities comparable with that of water, but presumably they also must once have passed through a stage of extreme rarefaction. Yet we know of no stars of small weight that are in a rarefied condition like the red giants; in fact the big gap, quite vacant of stars, lying between the rarefied red giants of great weight and the comparatively dense red dwarfs of small weight is one of the best established facts of sidereal astronomy.

It is generally accepted that all the stars on the main sequence have approximately the same central temperatures, about 20 million degrees. Moreover, as we have seen, the weights of the stars in the main sequence fall off regularly from the most massive white stars to the least massive red dwarfs. Presumably, therefore, the less massive a star is, the denser has it got to become—and hence the redder will be its colour and the lower its surface temperature—before its central temperature reaches that critical value common to all stars on the main sequence, at which the rapid transmutation of the elements comes into action. In other words the less massive a star is, the lower is the point on the main sequence at which it becomes stabilized: a star of small mass will be stabilized as a red dwarf, a slightly more massive star as an orange dwarf, and so on up to the most massive white stars.

Now the life of a star *as* a star starts from the time that it first becomes luminous. There are thus two reasons why the less massive a star is the smaller is our chance of catching it during the stage of rapid contraction that precedes its stabilization on the main sequence. In the first place a star of small mass becomes stabilized, as we have just seen, when its surface is at a comparatively low red heat; so its surface brightness must always be low. In the second place a star of small mass must, at each stage of its development, have a correspondingly small size. Thus both because of its low surface temperature and small size, it will necessarily be a faint star. Consequently the less massive a star is, the smaller is the chance of its being bright enough to be visible during the stage preceding its arrival on the main sequence; and therefore, as we should expect, the gap which separates the giants from the dwarfs is widest for the red stars and gradually disappears among the yellow and yellowish-white stars.

Hence the theory which derives the source of stellar energy from the transmutation of the elements, while it rejects the simple evolutionary interpretation of the spectral sequence, is still able to give an adequate explanation of that sequence. It states that the reason why the vast majority of the stars are to be found close to the main sequence is simply that the enormously greater part of their evolutionary history is spent upon it. Excluding the final stage as a white dwarf 99·999 per cent. of the life of a star is spent in the stabilized condition on the main sequence. The chances therefore are small of our catching a star in the act of passing through the preliminary stages of rapid contraction. The giant stars form the only notable exception to this rule; for of course the highly rarefied red and orange giant stars are still in the stages which precede the main sequence. But this is just the exception we should expect. These giants are vastly more luminous than the other stars and are thus to be seen at much greater distances. Hence the chances of our catching a star in the brief preliminary stage of contraction is much greater in the case of a giant than in the case of a dwarf.

There is, however, one characteristic of the giant sequence which still requires explanation. The red and orange giants are much more numerous than the yellow and yellowish white giants; in other words, there is a partial gap containing comparatively few stars separating the most rarefied giants from the hottest stars in the main sequence. Professor Russell has suggested that the liberation of sub-atomic energy—whether by annihilation of matter or by the transmutation of the elements—may be dependent not only on a very high temperature, but also to a less extent on a very low pressure. The red and orange giants are the only stars that become luminous at a very low pressure; and it may be that this particular combination of a moderately high temperature with an extremely low pressure may favour the liberation of enough sub-atomic energy to stabilize them partially at this stage. If this were so the passage of a very massive star through the giant sequence would be slowed up during the red and orange stages of very low pressure; with increasing pressure the supply of sub-atomic energy would fall off and the star would again contract rapidly through the yellow giant stage; finally the central temperature would reach the level at which sub-atomic energy was once more liberated, and the star would be stabilized on the main sequence as a white giant.

Another cause which possibly contributes to the production of the gap between the primitive giants and the stars of the main sequence was pointed out in 1925 by Sir James Jeans. Some years earlier he had made a detailed study of the mechanism by which a double star was formed and had shown that many of the known double stars had undoubtedly been produced by the division of a single star. According to this theory the components of a newly formed double star would have a much higher surface temperature and thus a much whiter colour than the star from which they were formed. Thus, if a giant star, which was situated just on the orange side of the gap separating it from the main sequence, were to divide into two, its two daughter stars would so to speak be flung across the gap and become white stars high up on the main sequence. This view receives considerable support from observation. It is found that a very large proportion of recently formed double stars—as judged from the fact that the components are still very close together—are white stars. Presumably if these stars had not divided they would have been yellow giants helping to obliterate the gap which now exists.

We must now summarize briefly the state of affairs in regard to stellar evolution and the ultimate source of stellar energy as it stood about 1930. The original giant-and-dwarf theory of stellar evolution with its delightful clear-cut simplicity had been superseded either by one of two alternative theories, each of which was highly elastic, or by an even more elastic combination of both. But though the situation was now much vaguer the advance in our knowledge was definite. It was now certain that the energy of stellar radiation must be derived in some way from the liberation of sub-atomic energy; but whether the process was one of annihilation or of transmutation or of both annihilation and transmutation had yet to be decided. Again, it seemed to be well established that the time-scale of the universe lay between a period of the order of ten million, million years and a period of the order of a hundred thousand million years; but which of the two was right would depend on whether annihilation or transmutation was the order of the universe. Finally there had been worked out two fairly definite schemes according to which a star might evolve —one in which it marched steadily up and down the spectral sequence, and one in which it remained at a particular stage in the sequence for most of its life. It was impossible, however, to choose between the two until the question " annihilation *versus* transmutation " had been settled. Up till about 1930 the majority of astronomers were on the whole inclined to favour the theory of annihilation. It gave perhaps the simpler interpretation of the spectral sequence; but above all it provided a time-scale which was long enough for equipartition of energy to come about. Since 1930 however the pendulum of opinion has tended to swing over to the other side.

The reason is that during the last few years three things have happened which bear directly upon this question. In the first place the evidence of the cosmic rays has become suspect and they have more or less had to retire from the witness-box. We saw that they were at one time thought to be produced by an electron annihilating a proton and were thus regarded as evidence in favour of the annihilation of matter. Others thought they were emitted when hydrogen was transmuted into helium. It seems now that their energy was grossly underestimated and that neither of these processes could have produced them. There is now evidence that instead of being a form of radiation they consist of high-speed particles. And so though the mystery surrounding the cosmic rays thickens, it is probable that they can be dismissed from consideration in the present problem.

The second event has been the realization by the Cambridge physicists of the age-long dream of the alchemist—the transmutation of the elements. In 1932 Dr. Cockroft and Dr. Walton succeeded for the first time in converting artificially a few of the lightest elements into elements of slightly higher atomic weight. Though their achievement would scarcely have satisfied the alchemist, whose interest in science was doubtless mainly financial, it gave every satisfaction to the astronomer whose chief desire was an experimental proof that transmutation was possible. Although the experiments in no way disproved the possibility of annihilation they did at least make an ignominious distinction : transmutation immediately became a " respectable theory," while annihilation continued as before under the stigma which attaches to all hypotheses. Another matter which has a bearing upon this subject is the recent theoretical investigation by Sir Arthur Eddington of the " no man's land " between the relativity and quantum theories. He finds that the total number of protons and electrons in the universe is unalterably fixed, the number of each being 10^{79}—or 10 multiplied by itself 79 times. This may sound like " hypothesis " with a vengeance, and there are many authorities who do not agree with him. Nevertheless it is interesting to note that on this hypothesis Sir Arthur Eddington has been able to predict, without any reference to astronomical data, such quantitative results as the rate at which the spiral nebulae are receding from the earth in close agreement with observational measurement. Needless to say, if this view is correct the bottom of the annihilation theory is completely knocked out.

But the third event offered the strongest argument against the long time-scale and the theory of annihilation which it demanded. We have seen how Dr. Hubble and Mr. Humason at Mount Wilson had been engaged on the measurement of the distances and the velocities of the spiral nebulae. By 1932 their results had converged upon the conclusion that the entire meta-galactic system to its furthest observable limits was expanding symmetrically at a prodigious speed, while the velocities with which the various nebulae were receding increased in proportion to their distance from us. But the vital point was that, unless one made the unwarranted assumption that the rate of this expansion had been increasing very rapidly in the past, there must have been a time not more than a hundred thousand million years ago when the various spiral nebulae would have been as close together as they could possibly be crammed. If this deduction from the observations was correct, then a hundred thousand million years must represent an upper limit to the past age of the universe. And this would exclude the possibility of the annihilation of matter which demands a past period about a hundred times as long. We have seen that the observed recession of the spiral nebulae—the so-called expansion of the universe—can be completely accounted for by the theory of relativity. It can also be explained equally well by a modified relativistic theory due to Professor Milne of Oxford. According to the original relativity theory the rate of expansion becomes slower as we go back into the past; and it is on this assumption that the epoch, before which the universe could not have been any smaller, comes out at a hundred thousand million years ago. This therefore fits in satisfactorily with the time-scale provided by the transmutation of the elements. Professor Milne's theory requires the expansion to have proceeded at a constant rate from the very beginning; it thus gives an even shorter period than the original relativity theory for the past history of the universe, and is thus even more seriously opposed to the hypo thesis of annihilation.

Unfortunately the assumption of the short time-scale required by these two theories of the " expanding universe ". and by the observational data for the recession of the galaxies comes into conflict with the studies based on the equipartition of energy which require the long time-scale. Some astronomers are in favour of the long time-scale and explain away either the theories or the observations relating to the recession of the spiral nebulae. For example, according to the relativity theory the universe might either contract or expand, and although if it once started expanding it would be expected to continue expanding, it is possible to conceive that under certain conditions it might expand and contract alternately. It is conceivable therefore that the universe is pulsating and that the present expansion is but a phase in its history. Others think it is possible that the shifts in the spectral lines from which the velocities of the spiral nebulae are determined may be capable of a different interpretation. We know that high pressures and strong gravitational fields will produce shifts in the spectral lines similar to those due to a motion away from the earth. Such effects can be readily excluded as explanations of the very large shift found in the spiral nebulae; but it is of course possible that there may be other effects which are not yet known to physics. Probably however the majority of astronomers are now inclined to favour the shorter of the two time-scales. They accept the orthodox interpretation of the spectral shift as indicating a rapid recession of the spiral nebulae, and most of them will regard it as a manifestation of the expansion of the universe in accordance either with the theory of relativity or with the theory of Professor Milne. In order to explain how the observed degree of equipartition of energy has come about in so short a time it has been suggested (1) that in the past the stellar system was more compact than at present and that close approaches of the stars were thus more frequent; (2) that even now we underestimate the number of dark and invisible stars and hence also the frequency of close approaches, and (3) that there may be other agencies at work producing the effects that are usually ascribed to repeated close approaches of the stars. The whole problem, which is perhaps the most fundamental and fascinating one in astronomy, is still very far from being settled. The battle of the time-scales has now raged for over ten years, and though the short time-scale seems to have gained a temporary advantage, it is by no means certain to which side the victory will ultimately go.

CHAPTER XIV

VARIABLE STARS

We must now take up again the story of the variable stars. They are most conveniently classified in five groups: (1) Eclipsing Variables, which are not intrinsically variable and are more correctly to be classed as double stars; (2) Cepheid Variables, which go through their fluctuations with perfect regularity in short periods, ranging from a few hours up to forty or fifty days, and in which the variations are probably the result of rhythmic pulsations of their surfaces; (3) Long-Period Variables, which undergo much larger fluctuations in periods around 300 days which are not perfectly regular; (4) Irregular Variables, in which the fluctuations are grossly irregular, or more or less completely at random; and (5) Novae, which once in their lifetime suffer unexpectedly one gigantic flare-up. We have already discussed at length the eclipsing variables and the Cepheids and said something about the novae; we shall now deal with variable stars in general and particularly with the long-period variables, the irregular variables and the more recent work on novae.

The study of variable stars falls into three categories: the discovery of variables; the observations for the determination of their period and the manner of their variation, and the investigation of their other physical characteristics; and finally the statistical and theoretical study of the observational data.

Variable stars may be discovered visually, photographically or spectroscopically. The earliest discoveries were naturally made by visual methods and were thus mainly of stars which were conspicuous objects to the naked eye. The first variable to be recognized as such was the star Mira in the constellation of the Whale. Mira is a red star which at maximum brightness may reach the 2nd magnitude and is then the brightest star in that part of the sky. At minimum it sinks far below the limits of naked eye visibility. Its period may lie anywhere between 320 and 370 days, and its range of variation is also far from constant. It is typical of the long-period variables; and its fluctuations were first described by Fabricius about 1596. It was not, however, until the end of the eighteenth century that any real beginning was made in the study of variables, when several were discovered and systematically studied by Pigott and Goodricke in England.

The science once started grew rapidly. In 1844 Argelander recognized as true variables only eighteen stars. Yet ten years later Pogson at Oxford published a list of 53 variables; while Schönfeld, Argelander's assistant and successor, published in 1865 a catalogue of 113 variables, and in 1875 a second catalogue of 165. By 1896 Chandler, an American amateur astronomer, was able to list nearly 400 stars which were definitely variable. Immediately after this the rate at which new variable stars were discovered was enormously increased by the institution by Pickering at Harvard of the photographic method. A series of two or more photographs taken on different dates of the same region of the sky are compared star by star. The majority of the images will be identical from one plate to the next, while the occasional images which differ from one plate to another will obviously be those of variable stars. At first the method was to place the two plates which were being compared side by side, and to dodge so to speak back and forth from one to the other to see which of the images, if any, had undergone any change. But that was laborious, and soon various devices were employed to facilitate the comparison. In one of these a *positive* copy was made of the plate taken on the first date, and this was superposed film to film on one of the *negatives* taken on a later date. For the stars which had not varied in brightness between the two exposures, the bright images on the *positive* exactly neutralized the dark images on the *negative*; but if any star had varied in brightness then its image would stand out bright or dark according to whether it had grown fainter or brighter by the time of the second exposure. Another method was to compare two negatives in a special type of stereoscope: so long as the stars had not varied between the two exposures and their images were identical on the two plates they appeared to lie upon a flat ground; but if one star had changed in brightness then immediately its image would appear to stand out from the others. Still later an even more ingenious method became available, the use of the so-called "blink-comparator." In this machine the two plates for comparison are mounted side by side, and by a system of mirrors or prisms the same portion of either of them can be brought into the field of view of a low-power microscope. The positions of the two plates are adjusted so that when they are viewed simultaneously the star-images on each of them are exactly superposed. By one or other of a number of devices the two plates are viewed alternately in quick succession. So long as the stars have not altered in brightness between the two exposures, one has the impression on looking into the microscope that one is examining a portion of a *single* plate; whereas actually corresponding portions of two identical plates are alternating in quick succession in front of the eye. If, however, any of the stars have varied in brightness then one will at once see it " winking " at one in the field of view; and literally it fairly hits one in the eye.

Photography assisted by these various devices enormously augmented the rate of discovery of variables; moreover, whereas previously variation had only been detected among the brighter stars it now became possible to discover variables among the faintest stars in the sky. An early result of the greatest importance in this connexion was the discovery, in the closing years of last century, by Bailey at the Harvard station at Arequipa, that many of the globular clusters fairly abounded in variable stars of the Cepheid variety. But a still more important contribution was made in 1908. In that year Miss Leavitt at Harvard started on the study of the photographs taken at Arequipa of the Magellanic Clouds and found them to contain Cepheid variables in enormous numbers. Within two years she had discovered nearly 1,800 such objects in the two clouds and noticed that remarkable relation between their periods and their brightnesses, to which we have already referred, known as the " period-luminosity " law. The reader will remember how many years later Professor Shapley, by applying the " period-luminosity law " to the Cepheids in the globular clusters, was able to make the first definite determinations of the distances of those remote stellar systems.

Yet another method of speeding up the discovery of variable stars was also invented at Harvard. It was known that the long period variables showed in general a characteristic type of spectrum. These stars are all red stars and have the typical M-type spectrum of red stars which is crossed by multitudes of fine dark lines arranged in a beautiful fluted pattern. But when these variables are near their maximum brightness there appear superposed upon the M-type spectrum a number of *bright* lines which are never found in red stars that are not variable. During the compilation at Harvard of the first Draper Catalogue of stellar spectra, between 1886 and 1890, the examination of the plates and the classification of the spectra were in the hands of Mrs. Fleming. She was quick to realize how readily the presence of a variable could be detected by its characteristic spectrum, and within a few years discovered more than 200 new variable stars by this method.

We thus see how much variable-star astronomy owes to photography and particularly to photography at Harvard. In 1896 there were about 400 known variables; yet by 1910 they numbered nearly 4,000. And of these nearly 3,000 were discovered at Harvard photographically: over 500 by Bailey in globular clusters; over 2,000 by Miss Leavitt mainly in the Magellanic Clouds; and over 200 by Mrs. Fleming by means of their spectra. Since then the discovery of variable stars has proceeded at a more leisurely rate. It is probable that few if any variables brighter than the 7th magnitude have escaped detection; and there is no point in multiplying endlessly the discovery of faint variables except in selected regions for statistical investigations and in regions of particular interest such as clusters and nebulae.

Before the advent of photography, when all the variable stars that had been discovered were comparatively bright objects, the great majority of them were of the long-period variety. It was only when the star clouds and globular clusters and more recently the spiral nebulae began to be investigated that the short-period variables of Cepheid type were found to be so numerous. This can only mean that at least in the nearer parts of our own stellar system Cepheid variables are much less numerous than long period variables. The frequency with which Cepheids are found in the distant Magellanic Clouds, globular clusters and spiral nebulae is partly accounted for by their very great absolute brightness which enables them to be seen at distances at which the fainter long-period variables and most other stars would be invisible. But this is certainly not the whole explanation; for as Bailey originally found, the percentage of the stars in a globular cluster which are of Cepheid type varies enormously from one cluster to another. In some clusters the Cepheids may comprise fifteen per cent. of all the stars that can be photographed individually; in others they may comprise less than one-tenth per cent. One suggestion is that the Cepheid type of variation represents a certain stage in the life-history of a star, and the proportion of the stars found with Cepheid characteristics in a given region of the universe will depend on the stage to which that region has developed.

We must now consider the methods of observing as opposed to the methods of discovering variable stars. We have already seen that in the observation of eclipsing and Cepheid variables a high degree of precision is desirable and that nowadays these stars are usually observed by photographic methods, with some form of visual photometer of the polarizing or wedge type, or when the greatest accuracy is required with a selenium or photo-electric cell. When, however, one is dealing with the long-period or irregular variables the matter is quite otherwise: with such stars it is not only impracticable but also unnecessary to employ methods which give the highest degree of precision.

In the case of an eclipsing or Cepheid variable, as soon as one has determined the characteristics of its light variation one has finished with it and can scratch it off one's list: for each period is exactly like each other period, and nothing is to be gained by endless repetition of the observations. But it is by no means the same with the long-period and irregular variables: the fluctuations of the irregular variables are more or less at random, and even those of the long-period variables never precisely repeat themselves. Therein lies the whole beauty of these stars: for one can go on observing them literally for ever. But there are several hundred of these stars and each of them ought to be observed at least once a week. Obviously, therefore, it is impracticable to employ methods which require very expensive apparatus and are extremely time-consuming: the observers and the money are not sufficient to go round. But fortunately, altogether apart from it being impracticable, it is quite unnecessary. It is generally believed that a scientific observation should attain to the greatest possible accuracy; but it is not so generally realized that it is scientifically immoral to attempt a higher degree of accuracy than the circumstances warrant. No one would be so foolish as to attempt to measure the length of the road from London to Edinburgh to within a small fraction of an inch; for, depending upon which part of the road one measured over, the loose stones and the small bumps and hollows in it might easily account for discordances of a great many yards. The fluctuations of a long-period variable amount to several magnitudes, and in range and period they are far from being exactly regular. Further, we know very little as to the cause of these fluctuations; and, so long as there is no reason to suppose that a study of the microscopic fluctuations would assist us in the problem, it is a pure extravagance to observe them. A physician is often greatly helped towards a diagnosis by a study of the shape of the temperature curve of his patient. It would be absurd to replace the clinical thermometer which reads to one-fifth of a degree by another reading to one-hundredth of a degree; for the minute fluctuations in the curve revealed by the latter would be quite without significance as regards the problem in hand. And so in the observation of the long-period and irregular variables it is quite sufficient to observe brightnesses to fifths or tenths of a magnitude; and this can be done, by a trained observer, by the direct visual comparison of the variable with stars which are close to it in the field of view and are nearly equal to it in brightness, and of which the magnitudes have previously been determined.

But although observations of this kind can be made very quickly, at the rate of about six an hour, it would be quite impossible for the limited number of professional astronomers to keep under regular observation all the long-period and irregular variables. To meet this difficulty Argelander had, as we saw, issued an appeal to the " friends of astronomy," and asked those private individuals who possessed telescopes to train themselves in the observation of variables and thus occupy their leisure usefully. About half a century later a similar appeal was made by Pickering, which resulted in the co-operation of many amateur astronomers in America, and later in the foundation in 1911 of the American Association of Variable Star Observers. In England a similar movement was by this time already well advanced. In 1890 the British Astronomical Association—a society composed mainly of amateurs—had been founded, and soon afterwards its section for the observation of variable stars had started the periodic publication of its results. There are now large numbers of amateur astronomers all over the world, many of them equipped with large telescopes, who devote their time to the observation of variables. The publication and analysis of these observations are mostly undertaken by the British, American and French variable star associations and the vast amount of material collected in this way is becoming increasingly valuable for the study of the nature and cause of stellar variation. But although the number of variable star observers is considerable and is steadily increasing, there are still not nearly enough to cope efficiently with more than a small proportion of all the known long-period and irregular variables. And so it is that astronomers are always anxious to interest the layman in astronomy, in the hope that he may be encouraged to take up this fascinating study as a hobby.

Extraordinarily little is known as to the cause and nature of stellar variability. Only in the case of eclipsing variables is our knowledge in any sense complete; but in them the variability is purely optical and the stars themselves are not true variable stars. There is strong evidence that the variation of the Cepheids is due to the rhythmical pulsation of their surfaces, and the mathematical theory of Professor Plummer and Sir Arthur Eddington gives a satisfactory account of the mechanism of that pulsation. But why this particular type of star should pulsate, and whether there is a similar mechanism underlying the variation of the long-period and irregular variables we do not know. Nevertheless a great deal of data has been collected in regard to these stars, and recent statistical studies are at last beginning to penetrate the obscurity which surrounds them.

One of the first investigations of this kind was made by the Rev. T. E. R. Phillips, of Headley, Surrey. If one plots on squared paper the brightness of a long-period variable star night by night, thus making a graph of its light variation, one obtains a periodic curve or series of waves which are approximately regular. But the successive cycles or waves are not precisely the same; for both their periods and shapes vary slightly. By taking, however, a large number of cycles one can construct what is called the "mean light curve," which is a sort of compromise, a curve representing the average shape and period of the various successive curves. Now the simplest kind of periodic curve or wave is a *sine-curve*, which is roughly the curve a small boy draws with chalk as he walks beside a fence raising and lowering his arm. But the mean light curve of a long-period variable is by no means a simple sine-curve: its ascent, when the star is brightening, will not have the same slope as its descent, when the star is on the decline; and there may be humps on the up-going or down-going parts of the curve. Now, however complex the shape of a curve may be, it is always possible to regard it as being made up entirely of simple sine-curves, each one with a different period and with a different "amplitude" or height, the irregularity of the complex curve being merely the result of the superposition, one on top of the other, of the perfectly regular curves.

In 1904 the late Professor H. H. Turner of Oxford pointed out that it was of interest to analyse by the mathematical method of "harmonic analysis" the complex mean light curves of the long-period variables into their simple component parts. The suggestion was that the fluctuations in a long-period variable might be the combined result of a number of perfectly simple periodic processes operating simultaneously inside the star; and that one would stand a better chance of arriving at a physical interpretation of those processes if one first of all disentangled their individual effects. In 1916 Mr. Phillips made a subtle modification of Turner's methods and carried out the analysis of some eighty long-period variables. It at once became obvious that as a result of the analysis all these stars fell into one of two distinct groups. We cannot discuss here the mathematical point upon which the distinction was effected, but it is interesting to note that the difference in the mathematical characteristics of the two groups of stars is clearly reflected in a difference in the general appearance of the light curves of the two groups. In Group 1 the stars take almost the same time in rising from minimum to maximum brightness as in falling from maximum to minimum; in Group 2 on the other hand the brightening takes place much more rapidly than the fading. Several theories have been put forward to account for the existence of these two groups, but so far none of them is satisfactory. Undoubtedly Mr. Phillips has succeeded in putting together a very considerable chunk of the jig-saw puzzle, but until more of it has been filled in the pattern upon it remains obscure.

The long-period variables are all red stars, and they are all absolutely very bright. Few of them are conspicuous objects to the naked eye; but that is because the majority of them are at great distances from the earth. Since they are red and absolutely very luminous they must all be red giants, and thus, according to orthodox views, they must be in an early stage of their development—large and diffuse bodies at a comparatively low temperature. In 1924 Professor Ludendorf, of Potsdam, showed that there was a close relationship between the redness of these stars, their period of variation and the size of their light fluctuations. The redder a long-period variable is, the longer is its period and the greater are the fluctuations in its light. Since the reddest stars are the youngest it follows that the more youthful they are the more unstable they are in their brightness. This discovery fits in satisfactorily with a suggestion of Professor Stratton of Cambridge that the variability of these stars may actually be due to their extreme youthfulness. If they have but recently condensed out of the primitive nebulous material and are still highly rarefied, the force of gravitation at their surfaces will be so weak as to be barely capable of holding them in stable equilibrium. At first the pulsations in the star are very large; but as the star grows older, gets less red and contracts in size, gravitation gains a better hold on the surface and the pulsations begin to die down. We shall see in a moment what evidence there is for the extreme youthfulness of these stars.

In 1924 Drs. Merrill and Strömberg at Mount Wilson showed that the long-period variables were all very much of the same candle-power. If placed side by side with our sun they would each outshine him about 100 times. Dr. Gerasimovic at Harvard in 1928 came to very similar conclusions but found that those long-period variables with rather shorter periods—between 100 and 200 days—were considerably brighter than those with the longer periods, and outshone the sun about 500 times. These candle-powers refer to the *maximum* brightness of these stars.

The fluctuations vary considerably from star to star, but on the average about ninety-nine per cent. of the light is lost at minimum brightness, which makes the candle-power at such times more or less comparable with that of the sun.

Perhaps the most interesting work which has been done recently on the long-period variables is the investigation of their temperatures and temperature changes. At first these stars were thought to have the same sort of surface temperatures as the ordinary red giant stars, in the neighbourhood of 3,000 degrees centigrade. But it was soon found that there were certain special circumstances which made the temperature estimates unreliable in their case. We saw that the most practicable method for determining the surface temperatures of the stars was to estimate their colours or more accurately their colour-indices. The difference between the visual and photographic magnitudes gave a measure both of the colour and of the temperature of the star—for the redder a star is the lower is its temperature. But in very cool stars the temperature is so low that actual chemical compounds are able to remain intact in their atmospheres; and these compounds give rise to such numerous and such closely spaced spectral lines as to obscure large portions of the spectrum. Moreover, this obscuration occurs to a large extent in the red part of the spectrum, with the result that the red part of the star's light is diminished and the star looks less red than it would do if the spectral lines were absent. Hence, in the coolest stars the temperature judged from the colour or colour-index comes out higher than it ought to.

In order to get round this difficulty Drs. Pettit and Nicholson started to use at Mount Wilson an instrument for measuring the radiations received from a star in all parts of the spectrum indiscriminately. They used what is known as a thermocouple. The sensitiveness of the ordinary photographic plate is limited to the blue and violet, and to parts of the ultra-violet regions of the spectrum; likewise the sensitiveness of the eye is limited to the portions of the spectrum between the red and the violet, to wit the visual region. But the thermocouple is not so limited; it responds to the photographic and visual radiations, to the ultra-violet radiations beyond the photographic range, and to the invisible infra-red radiations which extend far beyond the visible spectrum at its red end. In astronomical investigations, however, the extreme ultra-violet plays no part; it is completely absorbed by our atmosphere and never reaches our instruments. Thus in measuring starlight the thermocouple is concerned only with photographic, visual and infra-red, or heat, radiations; and since the infra-red part of the spectrum is far more extensive than the photographic and visual parts it is customary to say that the thermocouple measures the *heat* received from the stars.

By placing a thermocouple at the focus of the Mount Wilson 100-inch telescope and directing the telescope in turn on different stars Drs. Nicholson and Pettit were able to compare the amounts of radiation received from them. Now just as there are photographic and visual magnitude scales, so one can construct an analogous scale of " radiometric " or heat magnitudes. As before, the radiometric magnitude scale is determined by Pogson's Ratio, a star of the first radiometric magnitude sending out 2·512 times as much heat as a star of the second radiometric magnitude. Finally, the arbitrary zero-point of the scale is adjusted in exactly the same way as was that of the photographic scale, so that radiometric, photographic and visual magnitudes are all the same for white stars of spectral type A.

It will be remembered that the relative amounts of radiation in the different parts of the spectrum varied with the temperature of the body emitting the radiation. As the temperature fell from the white to the red stars so the greater part of the radiation shifted from the violet or photographic end of the spectrum towards the red end where the eye is sensitive but the plate insensitive. Consequently the colour-index, or the difference between the photographic and visual magnitudes, changed with and gave a measure of the star's temperature. Now if we consider, instead of the visual and photographic magnitudes, the visual and radiometric magnitudes, we find an exactly analogous effect. The differences between the visual and radiometric magnitudes is known as the " heat-index " of a star, and just like the colour-index it changes with and gives a measure of the temperature of a star. We saw that the colour-index failed as a temperature indicator in the case of the coolest stars, because the obscuration by absorption lines of the red end of the spectrum prevented, below a certain temperature, any further increase in the redness of a star and therefore in its colour-index. The heat-index, on the other hand, not only goes on increasing at these low temperatures but does so at an increasing speed. The explanation is simple: just below 4,000 degrees the region of maximum radiation begins to shift out of the visible portion of the spectrum into the invisible infra-red portion which only affects the thermocouple. Consequently quite a small drop in temperature produces a large increase of the radiometric magnitude relative to the visual magnitude, in other words, a rapid rise in heat-index.

The radiometric results of Drs. Pettit and Nicholson were full of interest. For the ordinary (non-variable) red giants they obtained heat-indices which gave temperatures in excellent agreement with those got from their colour-indices. But for the long-period variables, especially those with the longest periods, they found much larger heat-indices, showing that the temperatures were often considerably lower than was suggested by the colour-indices. The temperatures seem to range from about 2,800 degrees, for stars with periods of about 200 days, to about 2,300 degrees for stars with periods round 400 days. These large heat-indices may be regarded from another point of view: they show that these stars are much more efficient as heat-givers than as light-givers. Regarded as lighthouses we saw that the long-period variables were giants, regarded as radiators we must promote them to super-giants: they outshine the sun some hundreds of times, but they out-radiate the sun some thousands of times.

In the bright star Mira and in a number of other long-period variables Drs. Pettit and Nicholson have measured the heat-indices in different stages of the light-cycle. They find that when these stars fade to their minimum brightness their temperatures become even lower and may reach 1,650 degrees. This of course corresponds to a very high heat-index, which means that the heat-index rises rapidly as the star fades. Since the heat-index is the difference between the visual and the radiometric magnitudes it follows that when the star sinks to its minimum brightness its heat diminishes very much less than its light. In fact the average long-period variable at minimum has about one-hundredth part of its brightness at maximum, and yet it diminishes its heat radiation by only about one-half; it is therefore much more constant as a radiator than as a lighthouse.

The spectra of the long-period variables are of extreme interest but are so far very imperfectly understood. Essentially they consist of a continuous spectrum with large numbers of absorption lines often closely packed to form delicately fluted bands. These are due to chemical compounds or molecules and to those metallic vapours which give rise to lines at low temperatures; the spectral characteristics thus fully fit in with the radiometric measures in indicating an extremely low temperature for these stars. Furthermore, as the stars fade to their minimum brightness all these low-temperature lines increase in intensity as they should do with a further fall in temperature. It is interesting that the molecular lines are not the same in all the variables. There are three distinct groups of these stars. In the largest group the molecular lines are due to a compound of titanium and oxygen; in a second group they are due to carbon and compounds of carbon and nitrogen; while in a third group they are due to a compound of zirconium and oxygen. So far the spectral characteristics of the long period variables are the same as those of the non-variable stars of the same spectral type. But in about eighty-five per cent. of the long-period variables there appears a spectral characteristic found in no other red stars. This is the presence of a number of bright lines superposed upon the normal spectrum. It was this phenomenon of the presence of bright lines in the spectrum of red stars which was shown by Mrs. Fleming at Harvard to be so to speak pathognomonic of variability, and was used by her as a means of discovering several hundred variables during her examination of the spectrum plates taken for the Draper Catalogue.

The bright lines in the spectra of these stars are variable. As the star approaches its maximum light, bright hydrogen lines appear; they reach their greatest intensity shortly after the star's maximum and then slowly fade out. When the star is near its minimum brightness bright lines due to various metallic vapours make their appearance. No satisfactory explanation has yet been given to account for the bright lines in the variable stars. The solution of the problem is undoubtedly of prime importance for the whole question of stellar variability, and a great deal of work both observational and theoretical is now being carried out in the hope of arriving at it. Up to the present the greater part of the data available has been obtained by Dr. Merrill and Dr. Joy at Mount Wilson; by Dr. Merrill from his study of the spectra of numerous variables, and by Dr. Joy from his more detailed observations of the spectrum of the bright star Mira during the different phases of its light-cycle. One interesting point is that certain of the bright lines in the hydrogen series are fainter than they should be as judged by other members of the series. The reason is that these abnormally faint hydrogen lines are partially obscured by the dark absorption lines of other vapours which happen to lie nearly in the same part of the spectrum. This can only mean, as Dr. Shajn of the Simeis Observatory in Russia has recently shown, that the intensely hot gases responsible for these luminous outbursts must lie beneath the main atmosphere of the star which is producing the normal dark absorption lines.

The bright and dark lines in the spectra of these stars show displacements from their normal positions in the spectrum, some of which vary with the different phases of the light cycle. So far only the displacements of the dark lines have been at all satisfactorily explained. Dr. Joy has made a careful study of these in the case of Mira. They indicate an alternate approach towards and recession from the earth as in the case of the Cepheids, which suggests that like the Cepheids these stars are in a state of rhythmical pulsation. But whereas the Cepheids give out most light at the moment when they are *expanding* most rapidly, the long-period variables are brightest when they are *contracting* most rapidly. This curious difference has naturally made astronomers somewhat loath to be too dogmatic in interpreting the displacements as due to pulsations, though on the other hand there are certain further points which suggest a similarity between Cepheids and long-period variables. There are typical Cepheids with periods up to about forty days, and there are typical long-period variables with periods as short as 100 days. In the gap between these two groups there are a small number of stars which Professor Ludendorf suggests represent a transition stage. Furthermore, there is a well-known relation between the period of a Cepheid and its density; and this same relation is found to hold in the case of Mira and other long-period variables for which the figures are available.

An entirely different theory to account for the variability of these stars was proposed by Dr. Merrill in 1916. It is supposed that in the comparatively cool atmospheres of these stars opaque clouds would tend to form and dam back the radiation in the lower atmospheric layers. Eventually the temperature of these layers would rise to a point at which the overlying clouds would be dispersed and the heated gases pent up within would escape outwards. The temperature would then fall once more and the whole process would be repeated. Although the theory explains many of the facts it is not easily reconciled with the very considerable degree of regularity in the light variation of these stars. Some astronomers have suggested a compromise between the two theories: the variation due to a pulsation of the star is modified by a periodic veiling of its atmosphere.

We must now consider briefly the irregular variables. As might be expected we are even more in the air as to the cause of the variations of these stars than we are in the case of the long-period variables. Some of these stars vary in a completely irregular manner, but others fall into definite groups showing similar characteristics, and in these there are certain regularities mitigating the irregularities. In one of these groups, of which the star R Coronae Borealis is typical, the stars remain of uniform brightness for long periods and then unexpectedly undergo a rapid reduction in light and return after an indefinite period to their normal brilliance. The stars of this group all tend to congregate in the neighbourhood of the Milky Way, and Professor Ludendorf has suggested that their occasional loss of light may be the result of their periodic obscuration, as they travel through space, by opaque clouds of nebulosity. He also suggests that the bright lines seen in the spectra of these stars may be connected with the actual passage of the stars through nebulous clouds.

In a second group called the U Geminorum stars the light remains uniform for most of the time but now and again increases rapidly and then fades more slowly. The intervals between successive maxima, though very irregular, tend to lie between certain limits. In some members of this group larger and smaller maxima alternate with one another. Unlike the R Coronae stars the U Geminorum stars do not tend to favour the neighbourhood of the Milky Way. Professor Stratton has suggested that their periods of greater brightness may be due to outbursts similar to those which give rise to the prominences on the sun, only on a much larger scale; but why there should be this strange rule about the alternation of larger and smaller outbursts it is difficult to see.

One of the irregular variables, *α* Orionis or Betelgeuse, deserves special mention because owing to its great brightness it has been studied in a way not possible for other stars. The theory that variations in brightness may be the result of a rhythmical pulsation of a star as a whole has been advanced to account for the variability of the Cepheids and rather more tentatively for that of the long-period and some of the irregular variables. That theory was based on certain mathematical considerations and was supported by periodic oscillations in the spectral lines. Now although Betelgeuse is neither a Cepheid nor a long-period variable, but rather a particularly irregular member of the irregular variables, it is strangely enough the one star in which it has been possible to demonstrate, by direct measures with the interferometer, actual variations in its diameter. So far it has not been possible to decide whether or not the pulsations run parallel with the variations in brightness, because the observations are difficult and do not yet extend over a long enough period. But there is no doubt at all that the surface of this gigantic star is continually expanding and contracting in a most incredible manner: sometimes it is swollen to 300 times the diameter of the sun, at other times it is shrunk to only 200 times its diameter. We know, therefore, that pulsations, and very large ones, do occur in certain stars; and though this does not prove anything in regard to the pulsation theory of Cepheids, it does at least give considerably more plausibility to that theory.

CHAPTER XV

NEW STARS

In a previous chapter we said something about " new stars " in connexion with Nova Persei 1901 and the novae which immediately preceded it. Since 1901 several other novae have appeared and a great deal has been learned from them. But before dealing with these more recent investigations we must say something more about the way in which novae are discovered and about the frequency with which they appear.

During the three centuries that elapsed between Kepler's nova in 1604 and Nova Persei in 1901 only five " new stars " were seen and not a single one of them approached the first rank in brightness. Yet during the thirty-six years of the present century that have already passed there have been no less than eight comparatively bright " new stars," two far surpassing the 1st magnitude, two reaching the 1st magnitude, two reaching the 2nd magnitude, and two reaching the 4th and 7th magnitudes respectively. Now this increase in the frequency of new stars is very striking and cannot reasonably be ascribed to the fact that there have just happened to be more new stars of late. Undoubtedly the most important if not the whole reason for the increase is that there is now a much more efficient watch being kept upon the heavens. When one remembers that a nova loses most of its light at maximum brightness in a very few days, that the skies are often continuously overcast for long periods, and that comparatively few astronomers are well enough acquainted with the configuration of the stars to recognize a new one when they see it, one realizes how easily in the past even a bright nova might have blazed up without being noticed. But nowadays the chances of that happening are becoming rapidly less. The number of astronomers is increasing, and, still more important, they are becoming more evenly distributed over the earth's surface. If a nova is missed in Europe because of clouds it will probably be picked up in America or Asia; and the same argument applies with even more force to the southern hemisphere, where until recently astronomers were extremely scarce. Again there has of late been a big increase in the amount of astronomical work done with the naked eye and with field-glasses, and oddly enough it is with such modest equipment that " new stars " are most likely to be discovered. In the observation of a bright variable star the naked eye or a field-glass is used in order to compare its brightness with that of the stars in its neighbourhood. Naturally the observers who are doing this work acquire a considerable familiarity with their " comparison stars "; and if a " new star " appears in the vicinity of a bright variable it thus stands a good chance of being spotted by a variable-star observer even though it is a faint naked-eye object. A class of astronomers who work almost entirely with the naked eye are the observers of shooting stars; and some of these through long watching have gained an extraordinarily close acquaintance with the configurations of even the faintest naked-eye stars. Finally, certain people have made a special point of learning all the naked-eye stars in the vicinity of the Milky Way and take a look every clear night to see if any new object has appeared among them.

It is interesting that most of the discoverers of the nine latest " new stars " fall into one or other of these categories. Dr. Anderson of Edinburgh who discovered Nova Aurigae 1892 and Nova Persei 1901, and Mr. Watson of South Africa who discovered Nova Pictoris 1925 belong to the last group. Nova Cygni 1920 was discovered by Denning of Bristol, who was perhaps the greatest of all meteor observers; and Nova Herculis 1934 was discovered by Mr. Prentice who was then director of the Meteor Section of the British Astronomical Association. Nova Geminorum 1912 and Nova Lacertae 1936 were discovered by variable-star observers: the first by M. Enebo of Norway, and the second independently by M. Gomi of Japan, by Sig. Loreta of Bologna and by M. Nielsen of Denmark, who was at the time on board a ship en route to observe an eclipse of the sun. This leaves us with only two of the nine novae unaccounted for. Nova Aquilae 1918 was so bright that it was found independently by large numbers of people. And finally, nova Lacertae 1910 was found in a most unusual fashion by the late Rev. T. E. Espin while searching for new double stars with his large reflector. Coming upon a star with a remarkably red colour, he immediately looked at it with a spectroscope and spotted it at once as a nova by its characteristic spectrum.

We see from this that "new stars" are practically always discovered with the naked eye (or field-glasses) and practically never with large telescopes. The reason is obvious: it would be as foolish to search for " new stars " with large telescopes as it would be to hunt a thimble with a microscope. Yet the layman seems to find this difficult to understand; and when in 1934 Mr. Prentice discovered Nova Herculis, the press tended with one accord to give the impression that a naked-eye astronomer had had the good fortune to pick up an important crumb which the astronomers with large telescopes had by some curious oversight let drop. They stressed far less the remarkableness of the picking up than they did the curiousness of the dropping. The achievement was of course most remarkable, for the star was only of the 3rd magnitude when Mr. Prentice found it; but it would have been infinitely more remarkable if the nova had been found by the Astronomer Royal with the 28-inch telescope at Greenwich or by Dr. Hubble with the 100-inch at Mount Wilson.

Since the institution of routine astronomical photography a large number of considerably fainter novae—what we may call "photographic novae"—have been discovered. The photographic novae are usually discovered posthumously. They are found incidentally during the examination of photographs taken months or even years earlier and so have generally faded beyond the range of our telescopes before their discoveries are announced. The importance of the discovery of a nova is not, as some people suppose, simply that something new has been detected in the heavens, but is rather that astronomers all over the world are thereby able to set to straightway, with their telescopes and spectroscopes, upon the study of the phenomenon. The sooner after its outburst a nova is discovered and announced the more valuable is the discovery, for it is most desirable to start the detailed investigations as long as possible before the star reaches its maximum brightness. But although the posthumous discoveries of photographic novae are of no use for this purpose, they have as we shall see in a moment a special value of their own.

The photographic discovery of novae is usually made from plates taken with a prismatic camera, such as that used for the Draper Memorial Catalogue, in which the image of each star is drawn out into a short spectrum. Any nova that is present on the plate will give itself away by its characteristic spectrum of bright and dark lines. The first discovery of this kind was made at Harvard by Mrs. Fleming during her examination in October 1893 of a Draper plate exposed three months before; and in 1895 the same astronomer discovered a "new star" on a photograph taken eight years earlier. Since then a large number of novae have been discovered by this means; but it is quite obvious that for the *early* discovery of novae the method would not only be uneconomic but quite impracticable. Probably hundreds of plates would have to be exposed and examined on the chance of finding a single nova; and since the whole of the Milky Way region would have to be explored in this way at extremely frequent intervals, it is doubtful whether all the prismatic cameras in the world working full time would be sufficient. Nevertheless these photographic discoveries have their uses. Thus Bailey at Harvard was able to show from a statistical examination of the plates taken at that observatory that at least ten and probably as many as twenty novae appeared, and reached magnitude 9 or brighter every year. Again when a nova has been discovered on a photograph it is often possible to find other photographs of the region taken about the same time and obtain additional records of its spectrum. Thus it has been possible to get *skeleton* spectroscopic histories of large numbers of faint photographic novae; and these, by filling out our knowledge derived from the detailed study of the few bright novae, have helped considerably in the general investigation.

There are two other ways in which photography has helped us with the nova problem. It is clearly desirable to find out if possible in what sort of condition a nova was before it was discovered: how bright it was before the outburst and how long it took to flare up. Before the days of photography this was impossible, for the hand-made charts which were available showed only the brighter stars. But now when a nova appears one simply looks up photographs taken of the same region on previous occasions and sees at once how bright the nova was in its original quiescent state and whether it was shining with a steady or a variable light. Moreover one is generally able to get hold of plates taken quite a short time before discovery, and then one can often set fairly close limits to the date on which the outburst began and to the rate at which the brightening occurred.

The other service rendered by photography is in relation to a class of nova somewhat different from that which we have been considering. The novae so far discussed have all been situated in our own stellar system—as is immediately suggested by their well marked aggregation towards the vicinity of the Milky Way, and as has more recently been shown by direct measures of their distances. In the last few years the detailed photographic study with the great American reflectors of the spiral nebulae has revealed, in some of the nearer of these objects, a number of quite temporary stellar points which behave exactly like novae. In the Andromeda nebula, the nearest of all spirals, no less than sixty-seven of these temporary stars were found between 1909 and 1926.

They rise suddenly out of complete invisibility and then fade fairly rapidly away. They differ from ordinary novae only in their extreme faintness, for at maximum brightness they reach only to about the 17th magnitude. It is interesting that all the novae in a given spiral have approximately the same brightness at maximum. We have already referred to the importance of this fact in relation to the distances of the spiral nebulae, and we shall see presently that it gives us a very definite clue as to the explanation of the nova phenomenon. We need merely remind the reader that the extreme faintness of these so-called " extra-galactic novae " is fully accounted for by the excessive distances of the spiral nebulae; in all other respects they are exactly the same as the ordinary comparatively bright novae, which are situated in our own stellar system at much smaller distances from us. Since the extra-galactic novae are barely visible even in the 100-inch telescope their investigation has been conducted entirely by photographic methods. But there has been one exception. In 1885 there suddenly blazed up near the centre of the Andromeda nebula a star of the 7th magnitude. If, as is practically certain, the 17th magnitude novae of the Andromeda nebula are similar to our own galactic novae, then this unique object, ten thousand times more brilliant, must have been the outcome of a celestial catastrophe of a vastly higher order.[1]

We must now follow the investigations which have been made of the " new stars " that have appeared since Nova Persei 1901. Nova Lacertae 1910 did not help very much; since it was about 8th magnitude at discovery and rapidly grew fainter, no very detailed spectroscopic studies were possible. But from Nova Geminorum 1912 a large mass of information was obtained. Spectral plates were got in great numbers all over the world, but particularly at the Solar Physics Observatory, Cambridge, and at the Allegheny Observatory in Pennsylvania. All the available data were collected and discussed by Professor Stratton of Cambridge in 1920. The earliest spectrum obtained showed the dark absorption lines mainly of hydrogen strongly displaced towards the violet; and since the star was already beginning to decline traces of bright lines were just visible on the red side of the dark lines. A few days later both dark and bright lines stood out more strongly, while at the same time a second set of dark lines appeared even more displaced to the violet than the first set, thus duplicating the absorption spectrum. Slowly both sets of dark lines moved in towards the bright lines (whose centres occupied their normal positions) and thus became less and less displaced towards the violet; and as they moved in they grew less dark. And then a week after discovery yet a third set of dark lines appeared more displaced than either of the other two. But this third absorption spectrum was not quite the same as the first two; some lines were missing from it, others were added. In fact, whereas the first two spectra represented those of A-type or hydrogen stars the third was that of the hotter B-type stars.

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[1] This star and a few like it, which have been found from time to time in some of the more remote spiral nebulae, are now known as super-novae. See pp. 303—305.

An important feature of the investigation of Nova Geminorum was the careful measurement to which the large series of spectral plates was subjected. It was shown beyond all doubt that the displacement of the lines was really the result of a motion of the gases producing them outwards from the star. Although this had long been the accepted explanation there had always remained in the background the possibility that pressure or electrical effects might have been the real cause of the displacement. Fortunately displacements due to motion exhibit a feature which displacements due to other causes do not. For the displacement is directly proportional to the wave-length of the line displaced; the red lines are displaced more than the yellow, the yellow more than the blue, and the blue more than the violet. And this rule was beautifully exemplified in all three absorption spectra of Nova Geminorum. The same story was told in rather a different form by the bright-line spectrum. As in previous novae these bright lines were broadened into wide bands the centres of which corresponded approximately to the normal undisplaced positions of the lines of the responsible elements. But it was now found that the widths of these bright bands were again exactly proportional to their wave-lengths. This immediately suggested that the widening of the bands was also a manifestation of motion: motion simultaneously towards and away from the earth and also at right angles to the line joining the earth to the star.

From these investigations there appeared at last to be emerging a definite physical interpretation of the spectral phenomena. It seemed that after a nova had attained its maximum brilliance there occurred a succession of explosions which ejected symmetrically from the star a series of shells of glowing vapours each one with a somewhat higher velocity than the preceding one. If it was assumed that each shell was enormously large in comparison with the body of the star, then only those parts of the shells which were moving directly towards the earth would absorb the light from the body of the star. From each shell as it was ejected we should therefore get an absorption spectrum displaced towards the violet by an amount equivalent to the velocity of ejection of that shell. But each shell would also produce a bright-line emission spectrum; and since light is *emitted in all directions* from each portion of the shell, the bright-line spectrum would come to us not only from the part of the shell exactly in line with us and the body of the star, but also from the outlying parts of the shell on either side of the body of the star and, moreover, from the *back* of the shell on the *far* side of the body of the star. Thus the bright line spectrum ought to show every degree of displacement from one of rapid approach to one of rapid recession. For the part of the shell immediately between us and the body of the star would be approaching us with the full velocity of its ejection from the star; the part of the shell exactly on the opposite side of the body of the star would be receding from us with the same velocity; while the intermediate parts of the shell, those representing its periphery as we look at it, would be moving at right angles to the line joining us to the star and would thus be neither approaching nor receding. Thus each bright line should be made up of an infinite series of overlapping lines lying between two limits which represent the greatest displacement to red and violet respectively. Consequently each bright line should appear broadened into a band: the centre of it undisplaced from its normal position, and its two edges displaced towards the violet and the red by an amount representing the velocity of ejection towards us on the near side and away from us on the far side of the star.

Two questions may occur to one. First, why are not the dark lines broadened like the bright ones? The point is that the body of the star is minute compared with the expanding shell, and only the portion of the shell immediately between us and the body will be able to absorb its light. This part of the shell must be moving exactly towards us; while all other parts of the shell which are not moving exactly towards us will not lie in a line with us and the body of the star, and so cannot absorb its light. Secondly, how can the light emitted by the far side of the shell ever reach us and produce a widening of the bright band towards the red; surely it must be absorbed and rendered invisible by the near side of the shell? The answer is that a luminous gas only absorbs light which has the same wave-length as it emits itself. And since the wave-length of the near side of the shell is diminished by its approach and that of the far side of the shell is increased by its recession, the near side of the shell will be completely transparent to the light of slightly different wave-length coming from the far side.

Some three weeks after discovery the decline of Nova Geminorum began to be broken, as in the case of Nova Persei, by roughly periodic oscillations in brightness; and again the ups and downs in brightness were followed by variations in the spectrum. As the star brightened the bright lines faded and the dark lines stood out more strongly; as the star faded the process was reversed. Then slowly the spectrum became composed more and more of bright lines only; and lastly, the lines of the metals were gradually replaced by those typical of nebulae.

The greatest of all novae of modern times burst forth in the sky, and announced itself to the world by its brilliance, on the night of June 8, 1918. It was then almost of the 1st magnitude, and on the following night it outshone all the fixed stars that were visible, reaching magnitude *minus* one. It was afterwards found to have been caught on a Harvard plate of June 7 as a 6th magnitude star and again on a Heidelberg plate on June 5, when it was of its normal pre-outburst brightness about magnitude 10½. So the flare-up of this star was extremely rapid, for within less than four days it brightened up by 11½ magnitudes or about forty-thousand times. On the second night after discovery the nova started on a rapid decline which carried it within three weeks below the 3rd magnitude. It then faded more slowly and for some weeks it was subject to minor fluctuations of brightness. Finally, about seven months after discovery, it passed out of range of the naked eye.

During the rise to maximum the star was pure white in colour. As soon as the decline set in it became first yellowish and then pink; and finally there developed a wonderful deep cerise colour —an indefinable mixture of scarlet and blue—which persisted for many weeks. In the end, when it was no longer visible to the naked eye, its colour changed to greenish white, the outward sign of its nebular metamorphosis. There was nothing unusual in this colour sequence; it was simply that the great brilliance of Nova Aquilae impressed it with such vividness upon the observers. It enabled the eye to summarize superficially and at a glance what was given as a detailed analysis by the spectroscope. The white colour during the brightening corresponded to the continuous spectrum crossed by occasional absorption lines; the change through yellow and pink to cerise was due to the fading of the continuous spectrum and the coming up particularly of the bright red hydrogen line; the fully developed cerise, the curiously indefinable blue-red colour, was the result of the dominance of the red line of hydrogen and of certain blue lines of hydrogen and other elements; and finally the green colour followed naturally upon the coming into prominence of the green nebular lines.

Undoubtedly the war hampered to a large extent the study of this great nova. A lot was done, but certainly a lot more would have been done had it not been that the majority of European observatories were seriously understaffed during the summer of 1918. A great deal of spectroscopic information was obtained but mostly of a nature too technical for discussion here. Measurements of spectral lines produced further evidence in favour of Professor Stratton's results from Nova Geminorum that the displacements were due to actual motion of the gases. But the most important contributions to our knowledge which issued from Nova Aquilae were in relation to the earliest and latest stages of novae.

Nova Aquilae was the first, and in fact the only, " new star " to have its spectrum recorded in the pre-outburst stage. It was then a typical hydrogen- or A-type star. This observation is of the greatest importance because immediately afterwards when the star was rapidly brightening it gave the *atypical* A-type spectrum with the so-called " c-characteristic " discovered by Miss Maury. It will be remembered that these " c-stars " have spectral lines which are unusually sharp and narrow, a feature which has been shown to imply abnormally large size and great luminosity. The c-stars are often known as " super-giants," and the sharpness of their spectral lines is probably due to the abnormally low density of their atmospheres. Now the c-characteristic has been found in all novae during their phase of brightening irrespective of the spectral type to which they belonged. From that might have been inferred the general rule that in order to become a nova it was necessary to be born a super-giant. But now it was clear that the c-characteristic was only a guise assumed after outburst and due presumably to the enormous lowering of pressure consequent upon the expansion of the star.

It will be remembered that about sixteen years after the outburst of Nova Persei 1901 the nova, once more a very faint object, was found to have developed a minute nebulous disc. Exactly the same thing happened with Nova Aquilae; but whereas the nebulous disc of the earlier nova took sixteen years to reach appreciable dimensions that of the later one took rather less than one year. This difference in time corresponds roughly with the difference in the distances of the two objects; for the various direct and indirect methods of measuring their distances give for Nova Aquilae less than one-tenth of the distance of Nova Persei. Moreover, the disc of Nova Aquilae like that of Nova Persei was found to be expanding, and again the rate of expansion fitted in well with the velocities given earlier by the spectroscope for the expansion of the gaseous shells. But probably the most interesting observations of all were those which Dr. Moore and Dr. Shane of the Lick Observatory started to make in 1919. With an extremely powerful spectroscope they succeeded in photographing the spectrum of the nova, not only of its nebulous disc as a whole but of its individual portions. The displacements of the spectral lines were different for different parts of the disc; but the differences were not such as could be explained by a simple expansion of a gaseous sphere. There seemed to be some sort of turbulence superposed upon the expansion. But though the motions were complex they were far from chaotic; they showed in fact a remarkable symmetry, which suggested the presence in the gaseous shell of currents rotating in opposite directions simultaneously. What significance is to be attached to these motions it is impossible to say. The important thing is that they have been shown to exist. In the case of future novae when larger telescopes are available their detailed investigation will doubtless lead to a fuller understanding of the mechanism of a nova outburst.

The next bright nova was discovered in Cygnus in 1920 by Denning of Bristol when little brighter than the 4th magnitude. It rose to the 2nd magnitude and then faded rapidly, passing out of reach of the naked eye within less than a month. It was perfectly typical in its behaviour, and the observations which were got fully confirmed the results obtained from previous novae. One interesting feature was the extreme complexity of the absorption lines which consisted of as many as six sets with varying displacements towards the violet.

On the morning of May 25, 1925, a " new star " was discovered by Mr. R. Watson in South Africa. Mr. Watson, who was one of the independent discoverers of Nova Aquilae, is an amateur astronomer who is always keeping a careful watch on the sky for new objects; and through his vigilance and prompt notification of his discovery, he rendered to astronomy on this occasion a service of inestimable value. Nova Pictoris turned out to be a somewhat atypical nova; in fact it was its very atypicalness which enabled astronomers to learn from it so much in regard to the nova-problem. Its rise to maximum brightness was curiously protracted, occupying about fifteen days. It was therefore possible to make a uniquely detailed study of its spectrum and light changes during this usually brief and inadequately observed phase.

A magnificent series of observations was obtained at The Cape Observatory and afterwards discussed by Dr. Lunt and subsequently in greater detail by Dr. Spencer Jones, the present Astronomer Royal, who was at that time His Majesty's Astronomer at the Cape. The result of chief importance was that during the two weeks in which the star continued to grow brighter its spectrum remained essentially unchanged. As the Cape astronomers pointed out, this could only mean one thing. The brightness of a star can increase for one or other of two reasons. Either its temperature goes up so that each little piece of its surface emits a more and more intense light. Or its size increases so that there is more and more surface for it to emit light from. Since the spectrum of Nova Pictoris remained unaltered its surface temperature could not have changed, hence its increasing brightness must have been solely due to an expansion of the star. A similar conclusion had meanwhile been reached by Dr. Hartmann in Buenos Aires, who cabled the *Astronomische Nachrichten*, " Nova problem solved, the star swells up and bursts." Though the nova problem was still far from a complete solution, and though the thesis that the star swells up and bursts had been put forward seven years earlier by Mr. Evershed, it was true that the phenomena observed in Nova Pictoris had gone a long way towards substantiating the swelling.

From the observed rate of brightening during the first two weeks it was possible to work out the rate at which the star was increasing in size—the percentage by which its diameter increased each day. In addition the displacements of the absorption lines gave the velocity in *miles per second* with which the star was expanding. Then by combining the two results one could get the diameter of the nova at different stages of its brightening. Now it will be remembered how Professor Hertzsprung had shown that if one took the temperature of a star's surface (derived from its spectral type) in conjunction with its *apparent* brightness, one got a measure of the star's *apparent* diameter. Thus finally, from a knowledge of the nova's apparent diameter and of its actual diameter in miles, one could obtain at once, by dividing the first into the last, the distance of the nova from the earth.

Working along these lines Dr. Spencer Jones was able to show that during the first two weeks of observation, while the star was still brightening, the diameter of the star increased steadily each day by an amount equal to fourteen times the diameter of the sun, or about twelve million miles. Finally, on the fifteenth day when it reached its greatest brilliance its diameter came out at 400 times the sun's diameter. From this last figure, in the manner we have just indicated, Dr. Spencer Jones determined the distance of the nova. Now if this distance could be checked by an independent method and shown to be correct, there would be strong presumptive evidence that Dr. Spencer Jones' method was sound and that the nova had in fact been expanding in the way he had indicated. Actually two opportunities for checking the distance presented themselves, the first one almost immediately and the second after the lapse of a few years.

Very soon a number of photographs came to hand which had been taken quite incidentally of the nova long before its discovery. From these Dr. Spencer Jones was able to derive the brightness of the nova before its outburst, and thus work out from the distance he had already found the absolute brightness or candle-power in the pre-nova stage. In the same way from the observed brightness at maximum he worked out its absolute brightness or candle-power at the time of greatest brilliance. It was very satisfactory to find that the first figure agreed closely with the average absolute brightness of an ordinary dwarf star of the same spectral type as the nova; and that the second agreed closely with the average absolute maximum brightness of novae in general.

The second check was made possible by the discovery in 1928 at the Union Observatory, Johannesburg, by Dr. van den Bos and Mr. Finsen of a nebulous disc round the nova. The disc was not round and symmetrical as in Nova Aquilae, but consisted of two or even three brighter nuclei. Between 1928 and 1930 the same observers found the disc to be definitely expanding. Taking the rate at which the disc was observed to be expanding in conjunction with the earlier spectroscopic observations of the velocity with which the main gaseous shell was ejected, a distance was got for the nova which was in close agreement with that obtained by Dr. Spencer Jones.

The importance of these two sets of results was, as we have said, not so much that they afforded a check on the distance, but that they also afforded a check on Dr. Spencer Jones' method for finding the distance. They provided valuable confirmation of the various diameters which had been deduced for the star in the various stages of its brightening, and thus of the general thesis that a nova is a star that swells up.

So far the investigation was concerned only with the pre-maximum phase of the nova, when it was still giving a continuous spectrum and had not yet developed an emission spectrum with multiple absorption lines. The swelling in this stage was therefore not to be confused with the expansion, in the subsequent stage of fading, of shells of ejected gases which we have discussed in previous novae. This early swelling, first thoroughly investigated in Nova Pictoris, involved the actual body of the star; the star simply swelled and its surface expanded like that of a balloon in process of inflation. On the other hand the expanding shells observed in the fading stages of previous novae were thrown off *from* the surface of the star and consisted of comparatively cool gases through which the hotter and more intensely bright surface of the star would be visible. It seemed natural to identify the sudden change-over at maximum brightness, from simple swelling to gaseous ejection, with the onset of the second process referred to in Dr. Hartmann's telegram, the process of bursting. At maximum brightness the star arrives at a certain limiting and critical distension, the continuity of the surface is then broken and the explosive outward-streaming of gases begins.

At the time of maximum brightness Nova Pictoris went through the usual spectral changes. During the whole period of brightening the single set of absorption lines crossing the continuous spectrum indicated a uniform velocity of approach (or of expansion) of about seventy-two miles a second—giving as we have said a daily increase in diameter equal to fourteen solar diameters. Immediately after maximum, bright emission lines appeared on the red side of the absorption lines and grew steadily brighter and broader. In addition, two sets of absorption lines appeared one after the other indicating the presence of two shells of gas ejected respectively with velocities of 80 and 200 miles a second. At the same time the continuous spectrum began to fade at a rate that corresponded with the fading of the general light of the star.

The question next arose as to what was *now* happening to the body of the star which up to the time of maximum had been rapidly swelling. During the early days of the nova's decline its rate of fading gave very closely a measure of the rate of fading of the *body* of the star. That this was so could be seen at once from the spectrum: for the greater part of the light of the spectrum still came from its continuous background, and it is the light from the body of the star which is responsible for the continuous background of the spectrum. On the other hand one could no longer assume, as one had been able to do in the pre-maximum stage, that the temperature of the body of the star was still unchanging; because the continuous spectrum was so much broken up and distorted by the complex overlying emissions and absorptions of the gaseous shells, that it no longer served as a reliable indicator of temperature. It was thus impossible to say how much of the fading of the nova was due to a diminution in the size of the body of the star, and how much was due to a falling off of its temperature.

But by a most ingenious method—the details of which need not worry us—based on small progressive changes in the widths of the emission lines and on a corresponding progressive increase in the displacements of the absorption lines, Dr. Spencer Jones produced evidence to show that after maximum brightness the body of the star proceeded to contract at a rate which fully accounted for its rate of fading. The rate of contraction came out slightly slower than the previous rate of swelling, amounting to about twelve diameters of the sun per day.

Dr. Spencer Jones' work on the spectrum of Nova Pictoris undoubtedly did a great deal towards clearing up the rather hazy ideas about the mechanism of a nova-outburst. For some reason or other a star suddenly begins to swell up rapidly. On reaching a certain critical distension a succession of gaseous shells begins to be discharged from its surface. These shells expand radially with a constant velocity, while at the same time the body of the star starts to contract. In the early stages of this contraction there is probably but little change in the temperature of the star; but later on, as we shall see presently, when the nova passes through the nebular stage, the temperature of the star rises to an extremely high level.

Since Nova Pictoris there have been two bright novae, Nova Lacertae 1936 and Nova Herculis 1934. Practically no observations have as yet been published of Nova Lacertae. It was discovered on the eve of the total solar eclipse of June 19, rose to equality with the Pole Star and then faded rapidly. Of Nova Herculis the observations got were so numerous that many years must still elapse before their discussion can be completed.

Nova Herculis was the first bright nova visible in the northern hemisphere since Nova Cygni 1920. The enthusiasm which greeted its appearance was terrific; never in the history of astronomy has there been so much ado about a single star. It was discovered at 4.30 a.m. on the morning of December 13 by the amateur astronomer, Mr. Prentice of Stowmarket. Mr. Prentice, who was Director of the Meteor Section of the British Astronomical Association, had been up all night observing shooting stars when he decided to take a walk round and stretch his legs. Looking towards the north he immediately noticed the presence of a strange star in Hercules and at once hurried to the Post Office to telephone his discovery to the Royal Observatory at Greenwich.

The result of the promptness with which he acted was that that very same morning Mr. E. G. Martin, who was on duty at Greenwich, was able to obtain a spectrogram of the nova with the new 36-inch reflector of the observatory. By the following evening the news of the discovery had been cabled all over the world; and from then on practically every observatory which possessed the necessary spectroscopic equipment devoted all available fine nights to the photography of the nova's spectrum. In England it was at the Solar Physics Observatory, Cambridge, under the directorship of Professor Stratton, that the campaign against the nova was waged with special vehemence. Christmas leave was forgotten, or at least not taken; and night after night observers were ready waiting to take advantage of even the smallest gaps in the clouds. After maximum brightness the nova began to fade rapidly in the usual way; but four days later it brightened up again, and then proceeded to remain, with considerable fluctuations but without any general tendency to fade, in the neghbourhood of the 2nd or 3rd magnitude. Week after week the nova retained this level, and week after week astronomers worked at that high pressure which only the emergency of a bright nova, usually so short-lived, demands. It was even whispered in some quarters that Nova Herculis had outstayed its welcome. And then suddenly, a hundred and ten days after discovery, the star took a precipitous dive and dropped in the space of a month below the 13th magnitude. But still the apparition was not at an end. During the next five weeks the nova rose slowly to the 7th magnitude and there it remained, fading scarcely perceptibly, for more than a year.

Undoubtedly a great deal will be learned from the detailed study of the thousands of spectrograms which were secured of this nova; but as we have said, many years must elapse before that can happen. In general outline the spectral phenomena proceeded much as in previous novae. The nebular metamorphosis commenced when the star faded after its prolonged maximum, and was well developed when it rose again to a level of comparative brightness. The spectral transformation indicative of this change was accompanied by a corresponding alteration of colour: for the star, which in April faded out cerise, brightened in July a gorgeous emerald green. And meanwhile in June Dr. Kuiper with the 36-inch refractor of the Lick Observatory had detected the first signs of the nova's nebulous disc—or rather two brighter nuclei of it which he soon found to be gradually separating from one another.

So far we have not said much about the changes in the *type* of the various spectra that succeed one another during the development of a nova. During the rise to maximum the spectrum does not usually undergo much alteration. But in the case of both Nova Persei 1901 and Nova Herculis the B-type spectrum found on discovery gradually changed over to the later A-type as maximum approached. In such cases as it occurs, this transition to a slightly later spectral type would be compatible with a lowering of the surface temperature of the nova during the process of expansion. A transition to a somewhat later spectral type may also occur immediately after maximum. This was most pronounced in Nova Herculis which from giving an A-type spectrum just before maximum gave immediately afterwards an R-type spectrum, as is found in certain low temperature red stars. But most often the spectral type persists unchanged up to and beyond the maximum. When the successive absorption spectra with their different displacements begin to appear, the first one or two of them are usually of the same type as the original pre-maximum spectrum. Thereafter there is an almost universal tendency for each successive spectrum to be of a slightly earlier type than the preceding one. We saw how this happened in Nova Geminorum 1912, its third absorption spectrum being different from its first and second spectra: the first two spectra gave lines of hydrogen and of ionized metals (A-type), while the third gave in addition lines of helium, oxygen and nitrogen (B-type). This gradual reversion to an earlier type affects the bright-line as well as the dark-line spectra, and results presently in the development of an O-type, or Wolf-Rayet,[1] spectrum in which the typical lines are due to the *ionized* gases helium, oxygen and nitrogen. About this stage the characteristic nebular lines begin to appear; and later with the fading of the continuous spectrum the dark lines disappear and we are left with a bright line spectrum either purely of nebular type or a mixture of nebular and Wolf-Rayet types.

It follows that the sequence of events after maximum brightness is an indication of a gradually increasing degree of ionization. Except for the ultimate appearance of the nebular lines, this would be compatible either with an increasing temperature or with a diminishing density. As we shall see presently, the nebular lines, which were so long unidentified and ascribed to a hypothetical element "nebulium," have now been shown to be due to doubly and trebly ionized oxygen and nitrogen the electrons of which are executing jumps that are only possible when the gases are excessively rarefied. Presumably, therefore, the sequence of events so far described is the result of the ever diminishing density of the expanding gaseous shells.

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[1] The Wolf-Rayet spectrum, as pointed out on p. 202, is an O-type spectrum with bright lines. It is described in greater detail on p. 298-9.

Yet the nebular stage is not as was once thought the final stage in which the exploded nova remains without limit. Later on, and may be after several years, the nebular lines fade and ultimately disappear, leaving an O-type or Wolf-Rayet spectrum. In those novae in which a nebulous disc has been observed, the disc tends to fade with the passage of the years; and this presumably accounts for the gradual fading and disappearance of the nebular spectrum. On the other hand the Wolf-Rayet spectrum persists, and undoubtedly comes from the central star which by this time has returned to approximately its pre-outburst brightness. And this, as we shall see presently, has a most important bearing on the theory of novae and on their connexion with a certain type of gaseous nebula.

CHAPTER XVI

NEW STARS (*continued*)

WE have made brief reference to the extra-galactic novae discovered in spiral nebulae and saw that no less than sixty-seven of these objects were detected between 1909 and 1916 in the Andromeda nebula alone. Since then many more have been discovered; and up to the end of 1934 the number of them had reached 116 in the Andromeda nebula and ninety in the various other extra-galactic systems. The total number of extra-galactic novae is in fact more than twice as great as that of the ordinary galactic novae; for the number of these latter objects officially recognized up to 1934 as definite amounted to only eighty-five. If, therefore, we can assume that the extra-galactic and galactic novae are essentially the same the extra-galactic novae, through sheer weight of numbers, should be of great statistical value. But so far, are we sure that they really belong to the same class of object? As yet the evidence for this was based only on the similarity in the behaviour of their light variations: their sudden appearance from invisibility, the rate of their subsequent fading, and the fact that there was only a single flare-up for each object. Owing to their extreme faintness nothing was known about their spectra. In 1929 Mr. Humason at Mount Wilson started with the 100-inch reflector to keep a strict photographic look-out for the appearance in the Andromeda nebula of a nova sufficiently bright for its spectrum to be photographed. The great majority of these objects were too faint for the purpose, but occasionally one would appear slightly brighter than the average; and he hoped that one of these might just come within reach of the very rapid spectrograph recently built for the telescope. After waiting three years the opportunity occurred, for in 1932 two novae somewhat brighter than their fellows appeared in the nebula. Exposures of a great many hours were given; and in each case, despite the minute size of the spectra—which is the necessary price paid for extreme photographic speed—he was able to identify the typical nova spectrum: the broad bright hydrogen bands accompanied on their violet sides by absorption lines. Henceforth it could be fairly safely assumed that galactic and extra-galactic novae were the same.

Now this conclusion that the extra-galactic and galactic novae are essentially the same is important in that it supplies us with a powerful weapon for dealing with the absolute magnitudes of novae. Not only does it give us, as we shall see in a moment, a perfectly homogenous group of 116 novae of which the magnitudes can be compared directly one with another, but it also provides evidence more reliable than any other of the *absolute* magnitude of these objects at their maxima. It may sound odd but we know the distance of the Andromeda nebula, and thus of all the novae within it, more accurately than that of any of the much nearer galactic novae. We have already discussed the various clear-cut lines of investigation that have converged unanimously on a distance of about a million light-years for the Andromeda nebula; and now we must summarize the various less definite methods through which estimates of the distances of some of the galactic novae have been obtained.

None of the galactic novae has been near enough to the earth for the direct trigonometrical method to give more than the very vaguest idea of its distance. Of the indirect methods for determining their distances we have already referred to two: the correlation of the velocity, given in miles per second by the displaced spectral lines, with the rate of growth of the angular diameter of the nebulous disc (page 265); and Dr. Spencer Jones' correlation of the velocity of expansion, given by the absorption spectrum before maximum, with the rate of brightening of the nova during the same period (page 264). But both methods depend on certain assumptions, so that the results although they are not likely to lead us far astray cannot be absolutely relied upon to give more than the general *order* of distance. Just recently a third method has come into use. Some thirty years ago it was noticed that in certain spectroscopic double stars, in which the spectral lines oscillated back and forth as the components alternately approached and receded from the earth, there might be lines of sodium and ionized calcium which remained fixed. Clearly therefore the gases responsible for these lines did not partake of the orbital motion of either star. Another point was that the stars in which these fixed lines were found were mostly early type (high temperature) stars with broad and diffuse spectral lines, whereas the fixed lines were always sharp and narrow. There were thus two reasons why the fixed lines should not come from the star's atmosphere proper; and it only remained to decide whether the gaseous cloud producing them was in the neighbourhood of the star and dependent upon it, or whether it was dispersed uniformly throughout the galactic system and thus quite independent of the star. It was as a result of the investigations of Dr. Otto Struve at the Yerkes Observatory and of Professor J. S. Plaskett of the Dominion Observatory, Victoria, B.C., that this matter was settled. They showed that the intensity of these fixed absorption lines became greater as the distance of the stars increased, just as the reddening of a street lamp by a fog becomes greater with increasing distance of the lamp; and later they made use of this principle for determining the distances of such stars as gave the fixed lines. Similar narrow lines of sodium and ionized calcium, not partaking of the violet-shift of the other absorption lines, have long been recognized as an almost constant accompaniment of the nova phenomenon. Thus the intensity of the fixed lines has given another method for checking up on the distances of the galactic novae. But unfortunately there are serious pitfalls, because the intensity of these lines may be spuriously enhanced by the superposition upon them of lines which are due to other elements and belong to one or other of the displaced spectra. So although there are several indirect methods for estimating the distances of the galactic novae—and there are still others we have not mentioned—none of them is infallible and none can be relied upon to give more than an approximate result.

There are, as we have already implied, two sides to the nova-problem; and so far we have been mainly concerned with only one of them. We have obtained a fairly rational picture as to what happens when a nova flares up, but we have not obtained any satisfactory explanation as to what in the first place is the cause of the flare-up. Recently, however, considerable advances have been made in this second aspect of the problem, partly as a result of the mathematical investigations of Professor Milne of Oxford and partly as a result of certain observational data that have accumulated during the last ten years.

Theories as to the cause of a nova outburst fell naturally into two categories; those which put it down to some sort of celestial collision, and those which ascribed it to the spontaneous explosion of a star. Both types of theories date back to last century, but until very recently the collision theories were the most widely favoured. We saw that the direct collision of two stars was excluded on the grounds of improbability: two row-boats in the Pacific Ocean would be more pushed for room than are the stars in space; and the chances of two stars colliding must be practically negligible. A variant on this theory was proposed by Professor W. H. Pickering, who suggested a collision between a star and a comparatively small body like a minor planet or a swarm of meteors. In this way the objection on the grounds of improbability was avoided, since such small objects must be very much more numerous than actual stars. The theory supposed that the heat engendered by the impact might bring about the liberation of the energy stored up inside the atoms and thus give rise to a conflagration out of all proportion in importance to the event which originated it.

But the type of collision theory which carried with it the most conviction, and which until a few years ago was the most favoured of all nova theories, was the one already referred to in which the outburst was regarded as due to the passage of a star through a nebula. The theory was first put forward by Seeliger in 1892 and was subsequently developed in greater detail by himself and Dr. J. Halm of the Cape.

On the other hand the theories which postulated the spontaneous explosion of a star suffered from vagueness. They could offer no very adequate explanation as to how a solitary conflagration of such magnitude could be brought about except to suggest that it might be similar to the outbursts of spots and prominences seen on the sun, only upon a vastly larger scale.

Probably the most striking thing about the novae is their remarkable similarity one to another: the excessively sudden flare-up followed by the much more gradual, though still rapid, subsidence; and even the few, which are not in this sense true to type, fall into a subsidiary group that is fairly well defined. Examples of this subsidiary group include Nova Pictoris, and particularly Nova Aurigae 1892 and Nova Herculis. In these the rise was less violent and broken into stages, while the maximum was greatly prolonged.

After the advent of photography, when, as we saw, it became possible to trace back the history of the star before discovery, another striking similarity became evident. It was found that the amount by which a nova brightened between its pre-outburst phase and its maximum was remarkably constant, the majority increasing by eleven to thirteen magnitudes, between about ten and a hundred thousand times. There have been several novae in which the amount of brightening fell outside these limits; some have brightened up by as little as eight magnitudes and others by as much as fifteen. But the tendency for the increase in brightness to lie between eleven and thirteen magnitudes is greater than one can ascribe to pure chance.

When the distances of the novae came to be estimated still another similarity was suspected. It was said that the candle-powers or absolute magnitudes of the novae at maximum brightness were all closely the same. This certainly was so in the case of the numerous novae observed at Mount Wilson in the Andromeda nebula. These objects, apart from minor variations one way or the other, all attain closely to the same maximum *apparent* magnitude; and since they are all at the same distance from the earth their maximum *absolute* magnitude must also be closely the same. As we have seen the distance of this nebula is known with a high degree of certainty; and from it Dr. Hubble finds that the average absolute magnitude of these novae at maximum is *minus* 5·7, nearly 20,000 times the candle-power of the sun.

But in the case of the ordinary galactic novae the uniformity of the absolute magnitudes at maximum was not so evident. It seemed rather that the wish had been father to the thought, for when in 1926 Professor Stratton collected the available *data* the figures came out literally " all over the place." But since then it has been possible to make more reliable estimates of the distances of some of the galactic novae; and if we confine our attention to those for which the best estimates are available the absolute magnitudes at maximum range between *minus* 5 and *minus* 9, with an average of *minus* 7·4. This slight difference in the average figures for the two groups and the smaller degree of uniformity in the figures for the galactic novae cannot be regarded very seriously: it could easily be ascribed to the considerable uncertainties in the distance estimates for the galactic group.

It would thus seem that one of the most important matters which any theory as to the cause of a nova outburst must take into account is the uniformity of the nova phenomenon both qualitative and quantitative. Most of the theories described account for the qualitative uniformity but fail to explain the quantitative uniformity. For instance an outburst due to a collision might be expected to lie within almost any range of magnitude, depending on the circumstances of the collision and the size of the body running into the star. The same objection applies to the theory of a star passing through a nebulous cloud; while in this case we should expect occasionally to have *repeated* outbursts as the star passed in and out of different parts of the nebula. The failure to meet this requirement of quantitative uniformity constitutes a serious objection to all the theories so far considered. But in 1930 a quite novel theory of the cause of a nova outburst was put forward by Professor Milne of Oxford; and this it would seem is capable of accounting satisfactorily for the observed uniformity of the phenomenon.

Professor Milne's theory is not primarily a theory of novae: it is a much wider theory—a theory of the internal constitution of the stars—which so to speak takes the nova phenomenon in its stride. After the discovery of the white dwarfs and the extremely dense matter of which they were composed, Sir Arthur Eddington pointed out a curious dilemma in which these stars would as it were find themselves. The extreme density of the matter was made possible by the almost complete degree of ionization of the atoms composing it; and that in its turn was originally the result of the excessively high internal temperature. Presumably, therefore, when the star cooled—as it inevitably must with the further radiation of energy—it would regain a more normal density and thus have to re-expand. Unfortunately this was impossible, because it could be shown that the star would then not have sufficient energy left to overcome the gravitational forces resisting the expansion. Sir Arthur said that he did not see how a star which had once got into this compressed condition was ever going to get out of it. The solution of this dilemma was found in 1926 by Professor R. H. Fowler of Cambridge, who attacked the question from the point of view of the recent developments of the quantum theory. He showed that the very dense gases composing the white dwarfs existed in what was actually a new physical state, known as "degeneracy." A degenerate gas differs from a perfect gas in the same sort of way that ice, water and steam differ from one another. Professor Fowler then investigated mathematically the properties of a gas in the degenerate state; and his work was later extended by Professor Sommerfeld of Munich. Professor Fowler showed that the answer to the puzzle propounded by Sir Arthur Eddington was that when a star was once in the degenerate state it never did get out of it. A white dwarf is a star in which the greater part of its bulk is in the degenerate state, only its outer layers remaining gaseous. Any further fall in temperature would result in the degenerate core increasing at the expense of the outer layer of perfect gas. Later the gaseous layer would disappear and the star become completely degenerate; the temperature would by then have fallen to zero and all radiation would have ceased, for the star would have passed into the final stage of a "black dwarf"—a dead invisible hulk.

Armed with these results of the investigations into the properties of matter in the degenerate state Professor Milne set out to study afresh the various possible models upon which a star could theoretically be constructed. Previous theory had assumed that stars other than the exceptional white dwarfs were built entirely out of one sort of matter—matter in the state of a perfect gas. It had now become necessary to take into account the possibility of their being built out of varying proportions of two sorts of matter—both degenerate and perfect gas; and the question arose as to how this would affect the well-known relation between mass and luminosity. On the theory of a single kind of matter the luminosity had been shown to depend to a close approximation only on the weight of a star: that is to say there was only one possible luminosity that a star of a given weight could possess. The result of Professor Milne's work was to show that the luminosity was no longer only dependent on the weight of a star but within certain limits was also dependent on the relative proportions of degenerate and perfect gas in its make-up.

We shall here consider only that part of Professor Milne's theory which bears upon the nova phenomenon. He found, in accordance with Sir Arthur Eddington's mass-luminosity law, that a star of a given weight built entirely out of a perfect gas could only have one value for its luminosity. But if that same star possessed at its centre a part of its material in the form of a degenerate core, the luminosity would necessarily be slightly greater. It can be shown that within certain limits, the greater the proportion of the star's material that is condensed into this central core, the greater will be the luminosity of the star.

One might have supposed that this was contrary to the Mass-Luminosity Law which observational data had so strikingly verified. But actually changes in the size of the central degenerate core affect the luminosity to only a minor extent, and one can still regard the Mass-Luminosity Law as holding good to a fairly close approximation for the majority of stars.

The application of this work to the nova phenomenon is as follows. So long as a star possesses a degenerate core of even minimal size it is perfectly stable: should its luminosity increase its central degenerate core would merely grow in size at the expense of the overlying shell of perfect gas; should its luminosity diminish the opposite thing would happen and part of the degenerate core would so to speak vaporize into the overlying shell of perfect gas. But so soon as the luminosity diminished to the point at which the central core was entirely dissipated into the shell—so soon in fact as the star became entirely composed of a perfect gas—the star would become perched on the edge of a metaphorical precipice. For Professor Milne found that directly a star, which possessed no degenerate core, still further diminished its luminosity, the interior would be unable to support the overlying weight, and the whole body of the star would collapse on itself and condense cataclysmically into the degenerate state. In other words a star which is entirely built of a perfect gas is unstable, and any diminution in its luminosity must result in its catastrophic metamorphosis into a white dwarf—a small star consisting mostly of degenerate core and covered by only a shallow layer of perfect gas.

But how is this theory of the sudden *collapse* of a gaseous star into a white dwarf to be squared with the observed expansion phenomena of a nova? Professor Milne points out that the sudden collapse of the body of the star will result in an enormous outpouring of energy which will blow off as an expanding shell the surface layers of the star. That will represent the first nova stage, that of the expanding surface. Then at a certain critical degree of distension the continuity of the expanding surface is broken; and while the ejected gases continue to expand in the form of shallow and more or less concentric shells, our spectroscopes (and later our telescopes) begin to see through them to the collapsing, main body of the star.

The theory explains well the phenomena actually observed and it also supplies us with a reason as to why it should occur. But in addition there are certain observational data that lend the theory numerical support. According to Professor Milne's work the luminosity of the collapsed nova, after the explosion has completely subsided, should be approximately equal to that of the original gaseous star. And this actually is what is found, for when a nova has ceased to fade and reached a more or less steady level of brightness its magnitude is generally about the same as it was before the outburst. Further as Professor Milne has pointed out our observational data ought to be able to tell us whether a nova in its final stage has in fact that extremely high density which is an essential requirement of his theory. Now all typical novae have in their final stages the so-called Wolf-Rayet type of spectrum which indicates an extremely high surface temperature probably not less than 40,000 degrees. This implies a surface of intense brilliancy. On the other hand all the available evidence goes to show that the surface temperature of a nova before outburst is much lower and of the order of 10,000 degrees, which in its turn implies a surface brightness of comparatively low intensity. Yet as we have seen the total brightness of a nova is roughly the same in its final stage as it was before its outburst; and the only way in which this can come about is that the size of the star, the area of its radiating surface, should be very much smaller in the final stage than it was in the pre-outburst stage. Finally, since a nova would lose only a negligible amount of its mass in the ejection of its gaseous shells, the same weight of material will be contained within the large pre-nova as in the much smaller star which finally results. Consequently the density of the nova in its end stage must be enormously greater than that of the original star. In this connexion Professor Milne gives us actual figures—the uncertainties in which cannot materially affect the results—and deduces from them that the densities of novae in the final stage must be about 10,000 times that of water, a figure quite comparable with the densities of some of the white dwarfs.

It is thus clear that Professor Milne's theory of novae receives considerable support from observational data of diverse kinds. Yet perhaps more important still is the ability of his theory to explain that remarkable uniformity exhibited by novae which we have previously emphasized. For by making a nova outburst a definite consequence of the evolutionary process it explains as no other theory does why all novae in their manifestations both qualitative and quantitative are so much alike. The theory of course will need much substantiation before it can be finally accepted; but in the meantime it is probably to be regarded as the most satisfactory theory yet propounded as a solution to the nova problem.

CHAPTER XVII

THE GALACTIC NEBULAE

We must now consider the Galactic or Gaseous Nebulae as opposed to the Extra-Galactic or Spiral Nebulae which we have already dealt with in some detail. Progress in the study of nebulae has led naturally to modifications in the terminology of the subject; and the persistence in current use of several diverse terminologies is undoubtedly a source of confusion to the uninitiated. It will thus be convenient at this point to trace quickly the stages through which the terminology has passed; for in that way we shall not only rid our minds of unnecessary confusion but also provide ourselves with a helpful summary of what we have previously discussed.

The Latin word *nebula* means literally a mist or vapour, and was originally used simply because it afforded an adequate description of the appearance of those misty patches of light in which existing telescopes failed to reveal any stellar structure. One of the problems with which the elder Herschel had been faced was whether objects which appeared to him nebulous might not with the larger telescopes of the future be resolved into compact swarms of tiny stars: whether in fact there was any fundamental distinction between the so-called nebulae and very remote star-clusters. From his observations Herschel had concluded that the distinction was a real one, and that some at least of the nebulae were extensive clouds of gas.

That this view was essentially correct was proved later by Huggins who was able to examine some of the brighter nebulae with a spectroscope and found them to give a spectrum typical of a glowing gas. Soon after this, however, a fresh complication had arisen; for some of the most nebulous-looking of the nebulae had turned out to give, not a gaseous spectrum, but a spectrum characteristic of integrated starlight.

Still later had come Rosse's visual discovery of the spiral structure shown by certain nebulae, and the later photographic work, notably of Keeler, which had demonstrated that nebulae with spiral structure were far commoner than nebulae of other forms. And then it was found that the nebulae which failed to give a gaseous spectrum were invariably of this spiral type.

Thus in the early part of the present century it was customary to talk about two main classes of nebulae: the gaseous nebulae and the spiral nebulae. The gaseous nebulae were often referred to as the " green nebulae " because of the greenish quality of their light seen in the telescope, which in its turn was the direct result of the two green *nebulium* lines that dominated their spectra. By contrast the spiral nebulae were often referred to as the " white nebulae," their white colour being due to the continuous spectrum of starlight. Finally, the two types were often distinguished by reference to their distribution in the sky; the gaseous nebulae were called the *galactic* nebulae because they hugged the plane of the Milky Way, and the spiral nebulae were called the *extra-galactic* nebulae because of their marked aversion to the Milky Way. Although each of these three terminologies is still in use we shall here henceforward employ as regards the green, gaseous or galactic nebulae only the last one—for as we shall see presently many of them are neither green nor entirely gaseous.

We have already followed the steps which led to the conclusion that the spiral nebulae were vast systems of stars situated at immense distances outside our system. We need here make only two further points in connexion with these extra-galactic nebulae before leaving them for the discussion of the galactic nebulae. First, though the term *nebula* is strictly a misnomer when applied to the spiral nebulae, its use in the literature has become so universal that we cannot afford to abandon it. Secondly, though the spiral nebulae are strictly speaking *stellar* and not *nebulous*, they are to be distinguished categorically from the star-clusters. Star-clusters whether of the open or globular type are essentially part of our own stellar system: the globular clusters are distributed more or less spherically about our galaxy upon its confines; and the open clusters are the subsystems of stars which together with the non-cluster stars and the galactic nebulae compose what we may call the equatorial plane of our galaxy—the Milky Way. On the other hand the spiral nebulae as we shall see later are other stellar systems, other galaxies similar to our own, and like it are composed of open and globular clusters, non-cluster stars, and nebulae of the " gaseous " type similar to our own galactic nebulae.

Galactic Nebulae are of two kinds: the Diffuse or Irregular Nebulae which as their name implies conform to no particular shape, and the Planetary Nebulae which present a more or less spherical shell of nebulous matter surrounding a central star. We shall deal first with the diffuse nebulae.

In size and form the diffuse nebulae show as much variety as do the clouds in the sky. They may appear in the telescope no larger than a minute wisp of light barely distinguishable from the point-like image of a star, or they may extend through the greater part of a whole constellation. They may appear through the telescope quite bright to the eye, or an exposure of many hours may be required for their feeble radiations to imprint themselves upon the plate. Some present a clear quite textureless expanse of luminosity; some have a flocculent, tufted structure; some show a most delicate lace-like arrangement. And this great variety in structure and luminosity is displayed not only in the different nebulae but in different parts of the same nebulae.

These nebulae, as we have said, adhere closely to the Milky Way. Many are situated actually within its boundaries, others lie close upon either side of it. In both cases there is almost always an intimate association of the nebulosity with comparatively bright stars.* Within the Milky Way the nebulosity is often mingled with those dense star clouds into which here and there the galaxy's general background of faint stars is locally piled up; but almost invariably there are a few much brighter stars seemingly involved in the nebulosity. Likewise in the less densely populated regions outside the galactic borders nebulosity is practically only found in immediate association with one or more comparatively bright stars. Thus we find wisps of nebulosity coiled like wreaths of smoke among the stars of the Pleiades, while the great nebula which is centred about the mid-star of the sword of Orion is found to envelope all the brighter stars which form the sword and belt of that constellation.

But there is another most important association of the diffuse nebulae. Long ago William Herschel had noticed with his telescopes how often in the immediate vicinity of a nebula the sky appeared to be swept clear of stars. But it was not until the coming of photography many years afterwards that the reality of this phenomenon was fully emphasized: often a portion of the sky immediately bordering on a diffuse nebula would be found quite empty of stars, while dark rifts cutting into the contours of the nebula would appear almost as projections upon the nebulosity of the surrounding darkness. The appearances suggested irresistibly that the surrounding darkness represented not merely an absence of stars but rather some opaque material which hid from view the stars and the portions of the nebula lying behind it.

Shortly after this, in 1889, Barnard had started to use comparatively small cameras capable of depicting large areas of the sky on a single plate and to secure in this manner pictures of long stretches of the Milky Way. We have seen how those photographs revealed numerous dark patches and streaks lying upon the starry background, and how Barnard had refuted the first idea that they represented tunnels through the galaxy down which we looked upon the emptiness beyond. It would have required too much of a coincidence to suppose that tunnels of such length should be so straight and in such perfect alignment with us that we could see directly through them. He suggested that they also were due to some form of opaque celestial cloud obscuring the more distant view. And in support of this Barnard's photographs showed that many of the dark patches were outlined by faint nebulous light, just as a black cloud standing in front of the sun may have its edges brightly illuminated.

These important researches of Barnard on the dark markings in the Milky Way were continued over more than thirty years and left no doubt that they were obscuring clouds closely related to the diffuse nebulae. He called them the " dark nebulae "; and we now regard the diffuse irregular nebulae as comprising both luminous and dark varieties, and exhibiting a large range both in brightness and opacity not only from one nebula to another but in different parts of the same nebula.

It used to be supposed that a fundamental characteristic of the diffuse nebulae was that they gave a typical gaseous spectrum consisting of a few isolated bright lines on a dark background. In this respect they resembled the planetary nebulae and differed radically from the spiral nebulae. It was true that only a small number of diffuse nebulae were bright enough for spectroscopic examination, but there was no reason to suppose that the fainter diffuse nebulae would be different. In the year 1912, however, Dr. Slipher managed to secure a long exposure photograph at the Lowell Observatory of the faint nebulosity which involves the stars of the Pleiades. The spectrum turned out to be a continuous one intersected by a number of dark absorption lines; but, even more remarkable, it was an exact replica of the spectrum of the stars involved in the nebulosity. By 1919 Dr. Slipher had collected six more nebulae with continuous spectra. Most of these spectra were closely similar to those of the involved stars, but in some a few bright emission lines accompanied the absorption lines. The conclusion seemed to be that the diffuse nebulae were not *always* composed of glowing gases; some must consist of non-luminous clouds lit up by the light of the involved stars, others appeared to shine partly by their own light and partly by reflected light.

During the next three years our knowledge of the diffuse nebulae was greatly advanced by the exhaustive investigations by Dr. Hubble at Mount Wilson which were published in 1922. With the great reflectors at his disposal he brought up the number of the observed spectra of diffuse nebulae from twenty to sixty-two—the number of continuous spectra being increased from seven to thirty-three, and the number of gaseous spectra from thirteen to twenty-nine. There was thus a reversal in the relative frequency of these two types; for whereas previously nebulae with gaseous spectra had been much the more numerous, those with continuous spectra were now slightly in excess. The reason for this was that the nebulae with continuous spectra could only be reached with the most modern and rapid spectrographs, and had thus been overlooked in the earlier surveys.

A second result of the investigation was to emphasize the association, already noticed, between nebulae and stars. With but one or two exceptions each of these sixty-two nebulae enveloped one or more comparatively bright stars; and the exceptions were reasonably explained by supposing the involved stars to be hidden from us by opaque portions of the nebula.

A third result was of greater interest. In practically every case the extent of the surrounding nebulosity was found to depend in a quite definite way on the brightness of the involved star: the brighter the star the more extensive the nebulosity. The nebulosity surrounding a star of the 1st magnitude would extend to a distance from the star equal to several times the moon's apparent diameter; while the nebulosity surrounding a star of the 13th magnitude would only reach to a distance from it equal to about a sixtieth of the moon's apparent diameter. The relation was precisely that which would hold were the nebulosity shining simply by reflecting the light of the involved star. There were, of course minor deviations, but these could be readily explained. On the one hand the involved star might be partially obscured by dark material and thus appear slightly fainter than the extent of the nebulosity would indicate. On the other hand the nebulous material itself might not be very extensive, in which case its failure to shine to the full theoretical distance from the star would be simply because it was not there to shine. The conclusion Dr. Hubble was able to draw from this mass of *data* was that the diffuse nebulae were never self-luminous, but obtained their light indirectly, in some way, from that of the involved stars.

That the remarkable relation between the extent of nebulosity and the brightness of the involved stars should hold for nebulae with a continuous spectrum was not in the least surprising. For ever since Dr. Slipher's earliest results the conviction had been growing that such nebulae shone simply by reflecting star-light. But that it should also hold for nebulae giving a gaseous spectrum was quite unexpected. Such nebulae were originally supposed to be gases glowing on their own account, although the question as to exactly how they glowed had always been somewhat of a stumbling block. For a gas to become self-luminous energy is required; but in order to produce the spectral emissions found in the nebulae, such as the lines of hydrogen, helium and ionized helium, it is necessary to supply the respective atoms with a very large amount of energy indeed. If the nebula was at a high enough temperature to supply this energy itself, then any opaque material in it, instead of being dark, would shine with an intense brilliance. To get round this difficulty it had vaguely been suggested that the rarefied gases in a nebula were excited to luminosity by some electrical process, much in the same way as gases in a vacuum tube are made to glow by the passage through them of an electric discharge. But now with Dr. Hubble's new results a much more rational interpretation was possible: it was clear that nebulae with gaseous spectra like those with continuous spectra derived their light from the stars involved in them; presumably therefore the energy required to excite the nebular atoms to emit light must be supplied to them by the radiations of the stars nearby.

The fourth and last of the results got by Dr. Hubble was perhaps the most important of all, for it helped towards a more complete understanding of the mechanism by which the nebulae obtained their light from the stars. It had been realized for some time that stars surrounded by nebulosity were among the hottest stars in the sky, that is to say they were white stars of the earliest spectral types O, B and A. It will be remembered that the hottest of all stars are of type O, and next to them come stars of types B and A respectively. For purposes of more precise spectral classification the types in the series, O, B, A, F, G, K, etc., are subdivided into tenths—stars intermediate between B and A, for example, being labelled B_1, B_2, B_3, B_4, etc. Dr. Hubble discovered that in all nebulae with a *gaseous* spectrum the exciting star had a spectrum of type B_1 or earlier, i.e., B_0 or one of the O subdivisions; while in all nebulae with *continuous* spectra the involved stars were of spectral type B_1 or later. In cases where the involved star was of type B_1, borderline cases, the nebular spectrum often contained both absorption and emission lines.

This discovery was of the greatest significance. We saw that a very considerable amount of energy would have to be fed into the atoms to enable them to produce the lines of the nebular spectrum. Now Professor Russell has pointed out that the nebular atoms might acquire the necessary amount of energy either by absorbing radiation of extremely short wave-length or by colliding with fast moving electrons. Very hot bodies radiate short waves and emit streams of electrons; and the hotter the body the shorter are the waves and the more rapidly do the electrons travel. Finally, since the hottest bodies in the universe are the O and B type stars they would naturally be expected to be the most probable source of radiation capable of exciting the nebulae to shine. So the discoveries of Dr. Hubble fit nicely into the theoretical picture: it is always the very hot O, B and A stars that are associated with the diffuse nebulae, and it is only the hottest of these—those hotter than the B_1 stars—that are capable of exciting a nebula to emit a bright line gaseous spectrum. Stars with temperatures lower than those of B_1 stars are incapable of exciting a gaseous spectrum, so the surrounding nebulosity shines only by reflecting or scattering the light of the star and thus gives a dark line spectrum.

In addition to the lines of hydrogen, helium and other known elements, there were in the nebular spectrum many lines which could not be traced to a recognized source. Moreover the unknown lines were among the most prominent and included the two conspicuous green lines originally detected by Huggins when he made the first observation of the gaseous spectrum of a nebula. It was at first supposed that the unknown lines were due to some element hitherto undiscovered in the laboratory; and this hypothetical element had been given the name of "nebulium." But for over sixty years nebulium remained a puzzle unsolved by the astronomer.

In 1911 Professor J. W. Nicholson attempted by means of the then current views on atomic theory to deduce from the lines of the nebular spectrum the structure of the nebulium atom. His results indicated an atom with four revolving electrons. But later it was found that they did not account for all the nebular lines, and, what was more serious, upset the table of "atomic numbers," which as we have seen is already full up and has no room for new elements of small atomic weight.

Meanwhile other views were beginning to be aired. Perhaps nebulium was not one new element but many? Or was it possible that nebulium was not a new element after all, but was one or more quite familiar elements existing in an unusual physical state? And it was this last view that the majority came gradually to adopt as being most likely to turn out correct.

It will be remembered that in 1913 Bohr's theory of the atom gave the first satisfactory explanation of spectral lines, connecting them up with the jumps of electrons from one atomic orbit to another. It accounted completely for the lines in the hydrogen spectrum and in the simpler spectra of a few other elements; but the working of it became quite unmanageable when one tried to cope with the highly complicated spectra of elements in which there were several revolving electrons.

During the last dozen years, however, atomic theory has developed with great rapidity, and the new theory of wave-mechanics which has superseded the original quantum theory is now able to explain the majority of even the most complex spectra in terms of atomic structure. One of the earliest services to astronomy of the new theory was its solution of the nebulium puzzle. In 1927 Dr. Bowen of the California Institute of Technology succeeded with consummate ingenuity in tracing practically all the nebulium lines to their source. They turned out to be produced by electron jumps occurring in perfectly familiar atoms, some of which were doubly or trebly ionized. The majority of the lines were due to oxygen and nitrogen, the main constituents of our own atmosphere.

The explanation why these lines have never been produced in the laboratory, and are in fact never likely to be, is particularly illuminating. Most of these lines are what are known as " forbidden lines ": they are due to electron jumps that are forbidden by the rules of wave-mechanics. Roughly speaking the chances of an electron getting into a state in which it will be capable of emitting one of these forbidden lines, is so small that the atom is certain to collide with another atom before the event can occur. In a nebula, however, the gas is so highly rarefied, and the atoms so few and far between, that the intervals between collisions are long enough to allow the " impossible " to happen. Yet even if we were able to produce in the laboratory a vacuum of the extreme degree of rarefication found in the nebulae, it would still be impossible to detect the nebular spectrum. For there would be so few atoms left in the vacuum tube that the light emitted by them would be much too faint to affect a photographic plate. It is only when we have a " vacuum tube " many billions of miles in thickness—in fact one of the diffuse nebulae—that we get in line with our spectrograph a sufficiency of atoms to produce a detectable line.

It is interesting, as Professor Russell has pointed out, that the sources of nebulium are found to be what are known as the *permanent* gases (oxygen, nitrogen, etc.) which only pass into the liquid and solid states at excessively low temperatures. This conforms with the generally accepted view that the temperature of the nebulae must be extremely low, far lower than that at which most substances would condense out of the gaseous state, and probably not far above the absolute zero of space. Thus the identification of nebulium carried with it new information as to the physical state of the nebulae: it proved that the density of their gases was enormously less than that of the most perfect laboratory vacuums, and it was compatible with their being at a very low temperature.

There is an independent type of investigation which also leads to excessively low densities for the nebulae and gives, moreover, a fairly definite figure as an upper limit to their densities. In 1892 Ranyard had pointed out that the nebula in Orion was so voluminous that, if its average density exceeded one ten-thousand-millionth of the density of air, its total weight would be so great that its gravitational attraction would be bound to produce appreciable effects on the motions of the stars in that region of the sky. Yet observation showed that these same stars were practically stationary in space. More recently long exposure photographs show that the extent of the Orion nebula is far greater than Ranyard ever supposed; and by using this new datum one can conclude that the average density of the nebula must be very much less than one million-millionth of the density of air and probably of the order of one million-million-million-millionth.

The same arguments have been extended to include the dark as well as the luminous diffuse nebulae, and lead to densities of the same extremely low order. And this brings one to the question as to how a cloud so highly rarefied can be so opaque as to cut out partly, or even wholly, the light of the stars behind it.

It is well known that the amount of obscuration produced by a cloud of small particles is dependent not only upon the density of the cloud but also on the size of the particles of which it is composed. Up to a certain point the smaller the particles the greater is the obscuring effect. When the particles are so small that their diameters are about equal to the wave-length of light the obscuration they produce is at its greatest. When the particles are still more finely divided the obscuring effect diminishes, and by the time they have been reduced to sizes comparable with those of the atoms and molecules of a gas their power to obscure is comparatively slight.

In 1920 from studies of the opaque nebulae in the constellation Taurus, Professor Pannekoek of Amsterdam showed that the degree of obscuration they produced, if due to gaseous atoms, would require them to have a density so high that it would lead to an impossibly large figure for their total weight. This difficulty could only be overcome by assuming the dark nebulae to be largely composed of fine dust, the particles of which were roughly of a diameter equal to a wave-length of light.

It is now generally believed that the nebulae are composed partly of gas and partly of dust. The degree of opacity, which varies from nebula to nebula and in different parts of the same nebula, depends on the amount of dust of the optimum degree of fineness which is present. In the case of nebulae with a gaseous spectrum, the nebula gases are excited to luminosity by the radiations of a nearby hot star. Finally, in the case of nebulae with a continuous spectrum, the light of some neighbouring star is scattered or reflected back to us partly by the *gases* and partly by the *dust* of the nebula.

The distances of the diffuse nebulae cannot be determined by the direct method of trigonometrical parallax, for their large apparent size and ill-defined outlines make it impossible to measure their positions in the sky with the requisite precision. For many of them, however, an approximate idea of the distance can be got indirectly. A rough estimate of the distances of the involved stars is obtained from their spectral types and apparent brightness; but it must be remembered that the accurate method of Professor Adams and Dr. Kohlschutter for finding distances from spectral characteristics and apparent brightness is not applicable to early type stars such as are found in nebulae. But one can get a rough idea; and obviously the distances of the nebulae will be the same as those of the involved stars. Another method is to compare the proper motions of the involved stars with their velocities in miles per second determined spectroscopically. This method which is a statistical one and is strictly only applicable to large groups of stars will also only give a general indication as to distance. Nevertheless by using several methods of this sort and comparing the results, fairly probable distances have been got for a number of diffuse nebulae by Professor Lundmark of Sweden and others. In the case of the dark obscuring nebulae a more reliable method is available. By counting the number of stars photographed in front of the obscured regions and comparing these figures with similar counts made well clear of the obscuration, one can get a good estimate of the distance at which the obscuring material is situated. Sometimes the problem is complicated by the dark nebula not being completely opaque, by the fact that it is only partially obscuring the stars beyond. In such circumstances counts of the numbers of stars of *different magnitudes* may lead to an estimate not only of the distance of the cloud but also of the degree of opacity which it possesses.

The distances found for the diffuse nebulae both luminous and dark, range from about 200 to 4,000 light years. Occasionally there is evidence for several clouds of different degrees of opacity lying one behind the other. A good example of this is the great " North America " nebula in Cygnus recently studied by Drs. H. Müller and Hufnagel of Berlin. The nearer cloud cuts down the light of the stars behind it by about half a magnitude and extends from a distance of 240 light-years to a distance of 650 light-years. The cloud behind it is more irregular in its light absorption and lies at a distance extending between 2,000 and 2,600 light-years.

From the estimates of the distances of these nebulae we can at once deduce estimates of their dimensions. Their diameters reach into tens or even hundreds of light-years, and the volume of the Great Nebula in Orion must be at least a hundred thousand cubic light-years.

Numerous attempts have been made to detect changes in the diffuse nebulae, either shifts of them bodily among the stars, or changes in their shapes due to the relative motion of their component parts. But though successes have been claimed from time to time they cannot definitely be accepted. Professor Curtis of Ann Arbor who has carefully reviewed the whole subject says that any changes that have occurred in these objects during the last fifteen years are too small to be detected on the best available photographs. The only definite evidence of motion which we possess has been derived from the shifts in the spectral lines of the half dozen nebulae that are bright enough for the purpose. After allowing for the movement of the solar system among the stars the Orion nebula is found by this method to be practically stationary in space. The two nebulae with the most rapid motions are found to be receding from the solar system with the quite moderate velocity of about sixteen miles per second.

In the case of the Orion nebula which is the largest and brightest in the sky, it has been possible to study how the velocities of the gases, determined spectroscopically, differ in its various parts. This has been carried out by MM. Buisson, Fabry and Bourget in France and by Professor Campbell and Dr. Moore at Lick. The gaseous currents have velocities ranging from about five miles a second away from the earth to five miles a second towards the earth. The French observers felt that superposed on the generally irregular currents was a slight tendency for the nebula to rotate as a whole; but the Americans found no sufficient indication of rotation. And on the whole one would not expect to find much rhyme or reason in the movements of these quite irregular nebulous clouds.

Five or six nebulae are known or strongly suspected to vary in brightness. A portion of such a nebula will brighten up while perhaps another portion of it fades, the variations being quite irregular. These variable nebulae are small fan-shaped clouds enveloping a star which is itself irregularly variable, but the variations of the star and the cloud show no correlation. In every case the nebula is situated in a dark obscured region of the Milky Way. The simplest explanation is that wandering clouds of this obscuring matter are continually passing between us and the star and between the star and the clouds which it illuminates.

To summarize, the diffuse nebulae are voluminous clouds of gas and dust of extreme tenuity scattered widely and irregularly throughout that fundamental plane of our stellar system which is the Milky Way. They exist at a low temperature and only reveal their presence when they obscure the vista beyond or are lit up by hot stars in their vicinity. In form they are completely chaotic and the sluggish movements of their parts conform to no known rule or plan. And they show a remarkable addiction to the intimate company of the hottest stars in the universe. These are the bodies which for over a hundred years were thought to represent the primaeval stuff from which the stars were made, some of it having been left over in the process. But before discussing the evidence for that view we must say something about the other type of galactic nebula, the planetary nebula.

The planetary nebulae appear on photographs as rings of hazy light with a faintish star situated at the centre. What appears as a ring is actually a shell of luminous gas more or less spherical in form, the thickness of which is small compared with its diameter. When we look at such a shell the centre of it will naturally appear much fainter than the edges, because near the edges we shall be looking through a much greater thickness of the luminous material. In many of these nebulae the appearance is that of a disc rather than a ring, the centre of the disc being fainter than its outer parts.

The points of resemblance between the planetary and diffuse nebulae are as striking as the points in which they differ. In substance they are indistinguishable, yet in form there is no likeness between them. The planetary nebulae give a spectrum identical with the gaseous spectrum of the diffuse nebulae; and there is no doubt that in both types of nebulae the same gases are present under the same conditions of low temperature and extreme tenuity and are excited to luminosity in the same way by the involved star. On the other hand the simple symmetrical shapes of the planetary nebulae, conforming strictly to a set pattern, indicate a degree of organization in these bodies which is entirely lacking in the haphazard configurations of the diffuse nebulae. And just as a smoke-ring and a column of smoke rising from a forest differ utterly in their significance, so one may anticipate that the evolutionary status of these two sorts of nebulae need have little in common.

Rather less than 150 planetary nebulae are known, and since they are easily identified by their shape and spectrum it is improbable that there are many more within reach of our telescopes. In apparent size they vary considerably. The three largest have diameters between one-half and one-fifth the apparent diameter of the moon; but the majority are much smaller, and some are only to be distinguished from stars by their just appreciable discs and their gaseous spectra. They tend to crowd closely towards the Milky Way and are particularly numerous in one section of it. We have already seen that the solar system is situated at a considerable distance from the centre of the stellar system. And it is in the direction from us towards the centre of the stellar system that the planetary nebulae are most frequent. This is exactly what one would expect if the planetary nebulae were scattered evenly throughout the length and breadth of the stellar system but confined within fairly narrow limits above and below its fundamental plane, the Milky Way.

Another fact which fits in with this interpretation is that the planetary nebulae which are furthest from the Milky Way are generally those that appear the largest, while those that adhere most closely to it—especially in the direction of its greatest depth—are those which look the smallest. It is probable that within limits the planetary nebulae are roughly of the same real size, while the large variations in apparent size are mainly due to differences in distance. In that case we should expect the largest planetary nebulae sometimes to appear quite far from the Milky Way, while the smallest would always appear close to it and be most numerous in the part of it which lies in the direction of the centre of the stellar system. If one imagines oneself standing in a very large circular ballroom at some distance from its centre, the lights in it will be shining all round one: the brightest (nearest) lights will often appear quite high overhead; and the faintest (most distant) lights will generally be less high overhead, being in the further parts of the room where the perspective brings the ceiling nearest to the floor. Furthermore, the faintest lights will be most numerous towards the side of the room opposite to that on which one is standing.

The planetary nebulae often require a large scale photograph or large telescope to show up their ring-like appearance, while with smaller instruments or in small scale photographs they look simply like round hazy discs. In fact to the early observers they appeared rather like planets slightly out-of-focus, and were called " planetary nebulae " for that reason. In the largest of them a great deal of detail can be made out in the rings, as though there were several superposed shells of gas not perfectly concentric with one another. Many planetary nebulae appear elliptical rather than circular; and as we shall see presently it is probable that they are all somewhat flattened by rotation, and the elliptical or circular form is determined by the angle at which their axes of rotation are tipped towards us. There are also frequently deviations, sometimes considerable, from the perfect spheroidal form, as though the gaseous shells thinned out towards their equators.

In the large majority of planetary nebulae there is a faint central star, which may be either perfectly well defined and characteristically stellar, or somewhat diffuse and nebulous in appearance. In some of the cases in which no central star has been detected the nebulae are themselves so small that we should not be able to detect the star even if it was there. But in a few cases this explanation will not hold, and we are forced to suppose that for some reason or other the central star is too faint to be photographed.

Our knowledge of the distances of the planetary nebulae is very uncertain. Such indirect methods as are applicable to the diffuse nebulae are not available: for the planetary nebulae never produce any appreciable obscuration of the stars behind them, while the spectral type of the stars in the middle of them is so peculiar that it is impossible to make any direct deduction as to their absolute magnitudes. The distances of these nebulae can only be investigated by (1) the direct trigonometrical method of measuring their annual to and fro shift in the sky, and by (2) comparing their apparent motion among the stars over an interval of years with their spectroscopic velocities in miles per second. The first of these methods has been used extensively by Dr. van Maanen at

Mount Wilson, but the tiny shifts found are practically swamped by the observational errors. The second method has also been used by Dr. van Maanen as well as by others, and gives considerably larger distances. On the whole the distances seem to get larger with each new investigation. Dr. van Maanen's first results with the second method more than doubled the distances by the first method, while his later results with the second method more than doubled them again. Finally, in 1934 two independent investigations by Dr. Anderson at Mount Wilson and by Dr. Vorontsov-Velyaminov more than doubled the distances for a third time. The last observer studied no less than 119 nebulae and found for them distances ranging between 600 and 30,000 light-years. It is probable that the actual distances lie somewhere between these figures and a set of figures five or six times smaller.

Our knowledge of the dimensions of the planetary nebulae and of the absolute brightness of their central stars naturally depends on our knowledge of their distances and is subject to corresponding uncertainty. Dr. Vorontsov-Velyaminov finds diameters of one-fiftieth of a light-year for the smallest and of fifteen light-years for the largest of his series, while the mean diameter is a little over one light-year. As before these figures should be taken as giving an upper limit to the most probable diameters; and we cannot with certainty go further than to say that the diameters of these nebulae range somewhere between about ten times and about a thousand times the diameter of the orbit of the planet Neptune. Dr. Anderson, who gets rather similar results to Dr. Vorontsov-Velyaminov, also obtained evidence to corroborate the deduction, already made on general principles, that the nebulae which look smallest and appear nearest to the Milky Way are usually more remote than those which look largest and appear furthest from the Milky Way.

The average absolute brightness of the central stars of these nebulae comes out somewhere between less than ten times fainter than, and nearly a hundred times brighter than, that of the sun—depending on the distances we assume. But though such a vague conclusion might appear to be worth very little, the remarkable thing is that even if we adopt the highest of these figures, the stars are still very much fainter than we should expect. For the average absolute brightness of the O-type stars is nearly 5,000 times the brightness of the sun, and as we shall see in a moment the peculiar spectral characteristics of the central stars of the planetary nebulae suggest that their temperatures are very much higher even than those of the ordinary O-type stars.

By comparing photographs taken at intervals; efforts have been made to detect the occurrence of changes in these nebulae. With only one exception, the search has been in vain. In 1921 Dr. Lampland at the Lowell Observatory had definite suspicions of a change in the internal structure of the " Crab Nebula " in Taurus, and that same year Dr. Duncan measured some plates at Mount Wilson which settled the question beyond doubt. In the interval of eleven and a half years that separated the photographs certain portions of the nebula were found to have receded radially from its centre. But although, with this one exception, there is no evidence of internal motion to be got from direct observation, there is plenty of such evidence to be derived from the spectroscope. Professor Campbell and Drs. Moore and Wilson at the Lick Observatory have shown from the shifts in the spectral lines that the planetary nebulae are rotating, that the rotation is around the shorter of the two axes, and that the speed of rotation is more rapid the more elongated the nebulous ring is. This is exactly what would be expected if these nebulae were rotating shells of gas:—for rotation would produce through centrifugal force an equatorial bulge; and the more nearly edgewise the equator was directed towards us, the greater would be the rotational velocity as detected by the spectroscope, and the more elongated would the nebula appear. They also found that the layers of the gaseous shells nearest to the nuclei were rotating more rapidly than the outer layers. This suggests that the rotation is determined by the gravitational field of the nebula as a whole, the strength of which is greatest nearest to the centre. On this assumption Professor Curtis has calculated the weights of some of the planetary nebulae and finds them to range between four times and 150 times the weight of the sun. And since the greater part of this weight must be concentrated in the central stars, these stars must be extremely massive.

In addition to the displacements of spectral lines characteristic of simple rotation the Lick observers have detected curious distortions in the spectral lines which bear witness to the existence of other superimposed motions in the gaseous shells. One such outstanding distortion is a doubling of the lines: the two components being most widely separated towards the middle of the nebulous disc and merging again into a single line at the periphery. Taken in conjunction with Dr. Duncan's results from the direct photographs of the Crab nebula, this strongly suggests that the gaseous shells of the planetary nebulae are expanding rapidly outwards from their central stars—a striking analogy to the expanding nebulous discs of the novae. But in addition to the general expansion, the distortions show that there are complex currents localized in different parts of the nebulae and affecting the various gases differently; and this, it will be remembered, is just what Dr. Moore found in the nebulous disc of Nova Aquilae.

The spectrum of a planetary nebula consists of two parts very different from one another: the spectrum of the nebulous shell and that of the central star. The spectra of the nebulous shells are very similar to the gaseous spectra of the diffuse nebulae. There are minor differences, but these are no greater than the differences found among individual diffuse nebulae or among individual planetary nebulae—or even in different parts of the same diffuse or planetary nebula. These consist in variations in the relative brightness of the lines; and occasionally some of the lines are missing. Undoubtedly these variations depend in part on the temperatures of the exciting stars and on the distance separating the exciting star from the nebulous material; but there are probably other factors concerned such as the precise degree of rarefaction of the gases in the different nebulae.

The intensity of the light of a planetary nebula is much greater than that of a diffuse nebula; and for this reason they are much better subjects for the detailed study of the nebular spectrum. In fact the exact measurement of the positions of the nebular lines, which we owe mainly to the labours of Dr. W. H. Wright at Lick, have mostly been made on spectrograms of the brighter planetary nebulae; and it was these measurements that formed the principal data for Dr. Bowen's identifications of the " unknown " nebulium lines.

The ordinary method for photographing the spectrum of a celestial object is to use a large telescope to concentrate its light upon a narrow slit and then to photograph the slit through a prism. Each line in the spectrum is then an image of the slit. But instead (as we saw on page 66) one can simply put a large prism in front of a photographic telescope and proceed to photograph the object, such as a nebula, directly through the prism. One then gets a spectrum which consists not of a series of lines but of a series of images of the nebula, each image being a photograph of the nebula in the light emitted by one particular sort of interatomic electron jump. In other words, the images are pictures respectively of the hydrogen shell, the helium shell, the ionized helium shell, the singly, doubly and trebly ionized oxygen and nitrogen shells, etc., of the nebula.

As long ago as 1894 Professor Campbell pointed out that in the case of a certain bright nebula there were decided differences in the sizes of its various spectral images. More recently Dr. Wright has photographed by the above method the spectrum of numerous planetary nebulae and arranged in order of size the various spectral images. He found that on the whole the order of size was remarkably constant from one nebula to another. But it was not until 1927 when Dr. Bowen started to unravel the origins of the nebular lines that the significance of this discovery began to emerge. It was then seen that the lines which gave the largest images were those that were most easily excitable, and those that gave the smallest images were the least easily excitable. This was well illustrated in the cases where there were several images due to a single element in different stages of ionization: the higher the degree of ionization of the atom producing the image, the smaller that image would be. Thus it is found that the largest images are due to singly ionized oxygen; the next largest to doubly ionized oxygen and unionized helium; the next largest to ionized helium and other lines of doubly ionized oxygen; and the smallest to trebly ionized oxygen and quadruply ionized neon. Dr. Bowen has evolved a theory of the mechanism by which a nebula shines and this still further elucidates the phenomenon. The radiation from the exciting star is absorbed by the nebular atoms which thereby become ionized and lose one or more electrons. Subsequently the electrons return to the atoms, and as they fall back into the various atomic orbits they emit the corresponding spectral lines. But according to the theory, the degree of ionization (i.e., the number of electrons knocked out of the atom) must be greatest in the layers of nebulosity nearest the star and diminishes as one proceeds outwards into the external layers. Thus the more highly ionized atoms will be confined to the inner shells and give the smaller images while the less highly ionized atoms will extend into the outer shells and give the larger images. Dr. Bowen's theory requires for the hottest of the central stars of planetary nebulae temperatures ranging up to 100,000 and 150,000 degrees—enormously higher than those of any ordinary star.

A more elaborate method of studying the spectral images of the nebulae on these slitless spectrograms has been used in the last few years by Dr. Zanstra at the Dominion Observatory, Victoria, B.C. and by Dr. Berman at Lick. The negatives are examined with a self-recording photometer, which not only determines the relative sizes of the various images but also gives an automatic record of the distribution of the light in each of them. The data derived in this way have been used extensively both by Dr.

Zanstra and by Dr. Berman in the development of the theory of nebular excitation. And it is interesting that the temperatures they get for the central stars not only agree closely with one another for individual nebulae, but often reach the almost incredibly high figures suggested previously by Dr. Bowen.

Before saying anything about the curious spectra of the central stars of these nebulae we must notice how unexpectedly faint they are in comparison with their surrounding rings of nebulosity. We saw that Dr. Hubble had shown that in the case of a *diffuse nebula* its extent is dependent on the brightness of the involved star: the brighter the star the greater is the distance at which it can light up directly, or excite to luminosity, the surrounding nebula. Actually he went further than this, and got results indicating that the brightness of any portion of diffuse nebulosity was equivalent to the amount of light from the involved star which, at the distance in question, it would intercept. This of course was only reasonable: for whether nebulosity is directly lit up or indirectly excited, it could not theoretically reflect or re-emit more radiation than it received. But when Dr. Hubble extended the same investigation to the planetary nebulae the relation collapsed. The central stars were much too faint, or the surrounding nebulous rings were much too bright, by a factor often as great as 100. The solution of this apparent paradox was that the radiations of these central stars were much stronger than the telescope or camera led one to suppose. Presumably therefore the radiation was in the far ultra-violet part of the spectrum; for then, while it would be able to reach and excite the nebulosity, it would be quite unable to traverse the earth's atmosphere and so never be detected by our instruments.

The spectra of the central stars of the planetary nebulae are remarkable. We saw that the exciting star in a diffuse nebula had the spectrum of either an O-type or a B-type star. The central star of a planetary nebula has invariably the spectrum of the hotter O-type star, but in many cases it has an O-type spectrum of a peculiar kind. We saw that in keeping with their very high temperatures the O-type stars gave the lines of helium, ionized helium and various multiple-ionized elements. In most O-type stars these lines occur as dark absorption lines; but in a few, including many of the central stars of the planetary nebulae, some or all of these lines occur as bright emission lines. As far back as 1867 Wolf and Rayet at the Paris Observatory had found three small stars in Cygnus showing this mixed bright and dark line O-type spectrum. Others have since been discovered and now the so-called Wolf-Rayet stars number well over 200. About forty of these form the central stars of planetary nebulae but the majority are unassociated with nebulosity. We thus have planetary nebulae with no detectable central star, planetary nebulae with central stars giving either an ordinary O-type spectrum or a Wolf-Rayet spectrum, and finally the simple Wolf-Rayet stars unaccompanied by a nebulous ring.

Although one can arrange the Wolf-Rayet stars in a rough sequence from those in which only one or two of the spectral lines are bright to those in which the majority are bright, little is known definitely as to the significance of the sequence. We know that in general these bright-line stars are probably much hotter than the ordinary O-type stars, but it by no means follows that the temperatures are highest in those in which the bright lines are most numerous. Doubtless there are other more complicated factors involved, though temperature probably plays a part.

But there is another characteristic of the Wolf-Rayet spectrum, which at least in part is better understood. These stars have long been noted for the remarkable brightness of the ultra-violet part in comparison with the visual part of their spectrum. Almost certainly one explanation of this preponderance of the radiation in the ultra-violet is to be found in the high temperatures of these stars. We have seen that as the temperature of a luminous body rises the brightest part of its spectrum shifts gradually from the red end towards the blue and violet end. Already in the B-type and ordinary O-type stars the maximum brightness is shifted into the photographic region of the ultra-violet; and presumably in the still hotter Wolf-Rayet stars the effect is more marked, so that the preponderance of the ultra-violet over the rest of the continuous spectrum is still greater.

It follows that if only a star is hot enough most of its radiation will be in the extreme ultra-violet and for ever cut off from direct detection through the earth's atmosphere. But such a star would still be a most potent source of radiation; and though it might itself be excessively faint or even invisible on our best photographs, it would be able to excite and render visible or photographable surrounding nebulosity. It will now be seen that if the central stars of the planetary nebulae have themselves these extremely high temperatures, we get immediately an explanation of Dr. Hubble's paradox: that the planetary nebulae appear to radiate more than they receive from their central stars. Unfortunately observations of the continuous spectrum of these stars cannot decide the matter; for since the theory requires that the greater part of the radiation is undetectable, we obviously cannot measure what proportion of it is undetectable.

There is, however, supporting evidence available from a different direction. We have just referred to the recent studies of Dr. Zanstra and Dr. Berman of the spectral images of these nebulae and to their determinations, from the relative sizes and intensities of the images, of the temperatures of the exciting central stars. Both authors get the interesting result that the hotter the central star is the fainter it is found to be in comparison with the surrounding nebulous ring. In other words it is just as was suggested in the last paragraph: as the temperature increases and the radiation is pushed further into the ultra-violet, the proportion of the radiation capable of traversing the earth's atmosphere becomes less and less while the proportion capable of exciting the nebulosity increases. Dr. Berman goes a step further, and concludes that the reason some nebulae appear to have no central star is that the star is so hot that the amount of its radiation getting through the earth's atmosphere is not sufficient to be detected, although there is plenty of radiation in the extreme ultra-violet for the excitation of the nebula. Dr. Zanstra has determined the temperatures of the central stars of twenty-two nebulae. In the coolest, which has a temperature of 28,000 degrees, the star has about half the photographic brightness of the nebula; while in the hottest, with a temperature of 140,000 degrees, the star has only about one two-thousandth part of the photographic brightness of the nebula.

For some years it has been realized that the central stars of the planetary nebulae must be exceedingly dense bodies and possibly of the nature of White Dwarfs. Undoubtedly they are very massive stars; for we saw that Professor Curtis found the weights of the planetary nebulae to range between 4 and 150 times that of the sun, and there can be no question but that the greater part of this weight is concentrated in the central stars. Then we saw that the absolute brightness of these stars was most unexpectedly low in comparison with the usual absolute brightness of O-type stars; and when, finally, one takes into consideration that the temperatures of the central stars far exceed those of ordinary O-stars, the discrepancy in the absolute brightness is still further enhanced. It will be remembered that the most striking symptom of the white dwarf trait is this association of low absolute brightness with high surface temperature. For a very hot star must have an intensely brilliant surface, and if it is to be absolutely very faint it must necessarily be a very small star. The density then will have to be great, and particularly great if the star is abnormally massive.

It is true that we noted a tendency, in the last few years, to raise considerably the estimates of the absolute brightness of these stars; and this would imply a correspondingly greater size and lower density. But that tendency has been more than counteracted by the other tendency to increase the estimates of temperature. For there is a well-known relation connecting the density of a star with its absolute brightness and its surface temperature, and this shows that the density is much more sensitive to a change in temperature than to a change in brightness. In an investigation published in 1933 Dr. Vorontsov-Velyaminov found densities for the central stars of the planetary nebulae ranging from three times that of water, or twice that of the sun, to a million times that of water, or twenty times that of the white dwarf companion of Sirius. Yet he adopted for the absolute brightnesses figures considerably higher than most authorities would care to admit, and he made the very conservative assumption in regard to weight that each star weighed the same as the sun. Hence it is probable that in most of his cases the densities should be even higher. And so we may safely conclude that the central stars of the planetary nebulae are in general abnormally dense stars while many of them are typical white dwarfs.

When in 1930 Professor Milne put forward his theory that the outburst of a nova was the result of the sudden metamorphosis of a normal star into a white dwarf, he made the interesting suggestion that a planetary nebula represented the final state of a nova. We have already noticed some of the points of resemblance between a planetary nebula and the last stage of a nova, and we shall now summarize those points and add a few others.

(1) Several of the brighter novae have been seen to develop finally a nebulous disc which tend to resemble a small planetary nebula. (2) These nebulous discs like the nebulous portions of a planetary nebula give a bright-line spectrum typical of a nebula. (3) In both classes of bodies observation shows that the nebulosity is in the form of a shell of gas expanding radially outwards from the central star. (4) In both classes of objects the central star gives a characteristic Wolf-Rayet spectrum and exhibits typical white dwarf symptoms. The points of resemblance are in fact so striking that it is almost impossible to suppose that the mechanism by which the two classes of objects are produced is not fundamentally similar. On the other hand there are some very powerful arguments which seem to prove definitely that the *average* planetary nebula is not identical with the final stage of the *ordinary* nova.

The nebulous disc of a nova can actually be watched growing from year to year; yet a comparison of the best photographs taken over a period of fifteen years has failed to reveal perceptible change in any planetary nebula with the sole exception of the definitely atypical " Crab nebula." The velocities with which the gases in the nebulous discs of the novae are expanding radially are found spectroscopically to be of the order of several hundreds of miles a second; similar observations of the planetary nebulae give velocities for the expansion of their shells which seldom exceed fifty miles a second. The one exception is again the " Crab nebula " in which the velocities are comparable with those in novae. The oldest nova known that possesses a nebulous disc is Nova Persei 1901; and since its nebulosity has had much more time than that of any other nova to expand and open up, it presents a unique opportunity for examining the structural detail. Photographs of this nebula show that its outline is much more irregular than that of a typical planetary nebula; and though this is only one example there are good indications that the tiny nebulae surrounding some of the more recent novae are also lacking in that characteristic planetary symmetry. Again, it is interesting that the one planetary nebula which is particularly lacking in symmetry, and most nearly comparable to the nebula round Nova Persei, is the " Crab nebula." The nebulous disc of a nova would appear to be a somewhat transitory affair; for already that of Nova Aquilae has faded considerably during its process of expansion. It is certainly doubtful whether the discs of any of the known novae could ever achieve the degree of expansion found in the " Crab nebula " without fading into obscurity. In fact the shells of the planetary nebulae would appear to be built on a much more substantial plan than those of the novae; for if their shells were thicker, their prospects for survival would be better. But perhaps the most serious argument against the view that the planetary nebulae are the wrecks of ancient novae is that based upon the rarity of the planetary nebulae in space. Only 150 are known, and since they are so easily identified by their spectrum it is improbable that there are many more within reach of our present instruments. Professor Curtis has shown that the extreme rarity of the planetary nebulae among the stars can only mean, either that in the life-history of a star the planetary nebula-stage has a duration of only a few thousand years, or that a planetary nebula represents an excessively rare accident in stellar evolution. The first of these alternatives, though it would fit in with the rapid changes observed in the nebulosity round novae, is excluded by the complete failure to detect changes in the typical planetary nebulae. This forces us to adopt the second alternative; but we then see that it is quite impossible to identify the outburst of a bright nova, which evolutionarily speaking is very common, with the excessively rare accident that has only produced 150 planetary nebulae within reach of our instruments.

It would seem from what we have said that, whereas we probably cannot identify the wreck of a nova with the typical planetary nebula, we might reasonably identify it with the Crab nebula. If that is so, and if the Crab nebula has always been expanding at its present rate, the nova which produced it must have flared up in our skies over 1,000 years ago. Such a nova must have been of most exceptional magnitude—the result perhaps of the collapse of an abnormally massive star—otherwise its nebulosity would have dissipated itself long ago.[1] The rarity of such an occurrence is also indicated by the uniqueness of the Crab nebula itself—for we know of no other object like it.

But there is another possible explanation of the origin of the typical planetary nebula. In 1934 Drs. Baade and Zwicky suggested that the few super-novae, which have been observed in the spiral nebulae and which probably include the great nova of Tycho-Brahe situated in our own galaxy, are categorically different from the ordinary novae. Their suggestion is that in place of the formation of a large core of "degenerate matter," which according to Professor Milne's theory happens in the ordinary nova-outburst, there occurs in the production of a super-nova the transformation of the atoms into "neutrons" and the liberation of a vastly larger store of energy. (A neutron is a particle in which a proton and an electron are in close combination; it differs entirely from a hydrogen atom in which the electron is widely separated from the proton and free to jump from one electron orbit to another. But the neutron has only recently been discovered in the laboratory and its nature is by no means fully understood.) Dr. Baade and Dr. Zwicky point out that a super-nova radiates in a few weeks as much energy as the sun does in a million years, and they estimate that they occur about once every thousand years per galaxy. Now this is very much nearer the sort of frequency we should expect for whatever event it is that gives rise to the extremely rare planetary nebulae. Moreover since the outburst of a super-nova is vastly larger than that of an ordinary nova, it might be expected to give rise to a more substantial type of nebula, a nebula more akin to the planetary nebulae than to the nebulae originating from ordinary novae. The writer would therefore suggest that the planetary nebulae may possibly be the wrecks, not of ordinary novae, but of super-novae.

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[1] It has recently been suggested that this may have been the great nova which flared up in Taurus in July 1054.

It will be convenient to insert at this point a reference to some important work on super-novae, most of which has been done during the last few months. The spectrum of the Andromeda super-nova of 1885 was observed with difficulty and only visually. The first super-nova of which the spectrum was photographed was that which appeared in a nebula in Centaurus in 1895. Following that there was a gap of forty years before another such photograph was obtained. In the interval several super-novae were detected in the more distant spirals; but their spectra were not studied, either because they were too faint or because their discoveries on photographic plates were made " posthumously." At last in 1936, as a result of a systematic search of several years by Drs. Hubble, Baade and Zwicky at Mount Wilson, a super-nova was discovered which was bright enough (magnitude 17.5) for Dr. Humason to photograph its spectrum with an exposure of seventeen hours. It showed the bright lines of an ordinary nova, but they were enormously more widened than usual and indicated that the star was expanding at the rate of over 4,000 miles a second. About the same time Dr. W. A. Johnson at Harvard re-examined the old photograph of the spectrum of the super-nova of 1895, and found the same immense broadening of the bright lines.

Late in 1936 Drs. Baade and Zwicky started on a new systematic search for super-novae. The site selected was Mount Palomar, where the 200-inch telescope will ultimately be erected; and they used an 18-inch Schmidt telescope, which is a new type of reflecting telescope recently designed for photographing a large area of the sky on a single plate. They were rewarded during 1937 with the discovery of no less than three super-novae. One of them exceeded the ninth magnitude and thus enabled Dr. Humason with the 100-inch at Mount Wilson to obtain for the first time a really detailed photograph of the spectrum of a super-nova. The results of the spectroscopic studies are not yet published, but it is reported that the outstanding feature is again the enormous broadening of the bright lines. It is to be expected that this new and successful method of discovery will enable us during the next few years to learn a great deal about super-novae and their relation to ordinary novae. Meanwhile it would seem that the behaviour of the stars during their outburst is essentially the same in the two kinds of novae, except that in the super-nova the outburst is on a far larger scale and the expansion is much more rapid. Dr. Zwicky points out that the discovery of super-novae is important for another reason. Their absolute brightness at maximum is fairly constant from one to another and equal to several million suns; they therefore give us the distance of any galaxy in which they appear. And since their absolute brightness is comparable with that of an entire galaxy they can be seen to the furthest distances to which our telescopes can penetrate.

As regards the diffuse nebulae we can be perhaps slightly more definite about their evolutionary status. There have been three main theories. The oldest has a threefold source in the writings of the philosopher Kant, the mathematician Laplace, and the astronomer William Herschel; and it supposes that the diffuse nebulae are the birthplaces of the stars. The second regards the diffuse nebulae as the remnants of the primordial nebula which remained uncondensed and scattered widely through space after the stars themselves had crystallized out of it. The third considers the diffuse nebulae to be of comparatively recent appearance: the nebulous material is supposed to have been ejected from the hottest stars and to have gradually accumulated in the parts of the universe occupied by them.

The first and most romantic of these theories, after an innings of more than a century, can at last almost certainly be excluded. It was taken, as a point in support of this theory, that there were stars invariably involved in the nebulosity : that presumably such stars were in the actual process of being born. But we have seen that these stars are invariably the hottest stars, mainly of types O and B; and the one point on which all modern theories of stellar evolution are unanimous is that the O and B stars are far advanced in the evolutionary sequence. That we practically never find the young red stars associated with nebulae is thus the death blow to this theory. The *death blow*, however, is not to the theory that the stars originated from a primaeval nebula, but only to the theory that they are still originating from the diffuse nebulae.

The second theory, that the diffuse nebulae represent so to speak the rubbish left over at the creation, is faced with an objection which, though less serious, is considerable. Again, how is it able to account for the intimate relation between the nebulae and the white stars? Why should the waste material be so mysteriously swept into the vicinity of the white stars rather than of any other stars? One might counter this objection with the answer that since only the white stars can *excite* a nebula to shine, any nebulosity that was not in the vicinity of the white stars would be invisible. But against this we have to remember that some nebulae appear to shine by simple reflection, and a red star would be able to light up a nebula in this way just as well as a white star. Moreover, many nebulae reveal themselves by obscuring the background of the stars, and these would be as readily detectable in the vicinity of red stars as in the vicinity of white stars. Since therefore we are unable to dispose of the association of the nebulae with the white stars as being *apparent* rather than *real*, we have to admit that our second theory fails to explain one of the most remarkable facts about the nebulae.

This brings us to the last of our three theories, which supposes that the diffuse nebulae are the accumulations during millions of years of gaseous material driven out from the hottest stars under their intense radiation pressure. It obviously accounts at once for the association of the nebulae with the hottest stars, for the radiation pressure diminishes very rapidly with a fall in temperature. Now although we have no direct evidence that this is actually happening, we do know of a somewhat similar process on the sun occurring on a much smaller scale. The sun's corona which is only visible during a total eclipse consists of highly rarefied matter—atoms, electrons and dust particles—which is apparently being propelled outwards from the sun by radiation. Again, still further out beyond the corona is the Zodiacal Light, visible as a faint cone of luminosity just after dark stretching upwards from the sunset, or just before daybreak stretching upwards from the sunrise; and this appendage also is believed to consist of fine particles driven out of the sun by its radiation. It is therefore not unreasonable to suppose that in the hottest stars, where the radiation pressure is hundreds of times that of the sun, the same sort of phenomenon might be occurring on a large enough scale to account for the accumulation of the diffuse nebulae. Provisionally then, this seems to be the most plausible theory of the status of the diffuse nebulae.

In conclusion it is interesting to note how the theories of all types of gaseous nebulae—the nebulae of the novae, the planetary nebulae and the diffuse nebulae—are tending completely to reverse the old evolutionary order: nebulae to stars. There is a certain irony in the way in which these mysterious clouds, after masquerading in the old theories as the birthplaces of the stars, have been degraded first to the rubbish heap of the creation and finally to the still humbler state of mere stellar excrement.

CHAPTER XVIII

THE ABSORPTION OF LIGHT IN SPACE

IN recent years astronomical investigations of many diverse kinds have come to depend more and more on measurements of the apparent brightness of the stars. So many of the deductions made in regard to the heavenly bodies have to be based ultimately on a knowledge of their distances; and, except for a small minority of the nearer of them, their distances are most readily obtained by methods involving the measurement of apparent brightness. For a mere handful of the nearest stars the distances are found by the direct trigonometrical method. For the stars in some of the nearer " open clusters " which show convergent motions the distances are got by Boss' method. For the visual double stars that are found to be in orbital motion the method of dynamical parallax gives a close approximation to the distances. In this method the combined mass of the two stars is assumed to be twice that of the sun; and it so happens that any error in the assumed mass gives rise to a much smaller error in the calculated distance.[1] Finally, there is the statistical method which correlates the *apparent* velocity of a body *across* the sky with its spectroscopic velocity in *miles per second* in a direction *towards or away from us* in the sky; but this does not give one the distances of individual objects, but only the average distances of classes of objects.

Except for these, all methods used in the determination of the distances of the celestial bodies have the following underlying principle common to them. First, either from a study of the star's spectrum, or from a measurement of its colour, or (if a Cepheid star) from a determination of the period of its light variation, the astronomer makes a deduction as to its *absolute* brightness. He then measures its *apparent* brightness by direct observation; and finally by combining the two results he gets by simple arithmetic its distance. It is true that the distances of the more remote globular clusters and spiral nebulae have been found by comparing their apparent *sizes* with those of the nearer globular clusters and spiral nebulae of which the distances are known; but this was only possible after it had been proved, by methods involving the apparent brightness of the Cepheids in the nearest of them, that the actual dimensions differed very little in each of the two classes of body.

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[1] The calculation here is exactly the reverse of that described on page 109, in which the weights of the stars were determined when their distance from the earth was known.

Now any distance determination, involving the comparison of the absolute and apparent brightness, must assume as strictly true the well-known geometrical law that the brightness of an object varies inversely as the square of its distance. If there are two identical street lights, one twice as far away from us as the other, the nearer of the two will appear exactly four times as bright—but only provided it is a perfectly transparent night. If there is any mistiness, the further light will appear more than four times as faint as the nearer one by an amount depending on the thickness of the mist. If therefore we had failed to take account of the mistiness, and knowing that the lights were identical, had attempted to estimate their relative distances from us, we should have found the distance for the further light greater than it really was. It follows that any estimation of the distances of the heavenly bodies by this means depends for its accuracy on the perfect transparency of interstellar space. If there is any interstellar mistiness the distances will err on the large side, and more and more on the large side the further off the star in question is; and of course the errors will always be more serious in proportion as the mist is thicker.

Up to the end of last century it was more or less implicitly assumed that space was perfectly transparent, the reason being that it was as yet impossible to decide whether it was or was not, while it was easier to make the provisional assumption that it was. It is true that in 1847 the elder Struve believed he had found evidence for considerable interstellar absorption—much more than we now know to be possible—but his investigation does not seem to have been taken very seriously. Even when in 1889 Barnard started the photographic studies which led to his discovery of opaque nebulae in the Milky Way the obvious warning as to the possibility of a more widespread absorption was generally ignored. After all the dark nebulae with their sharply demarcated outlines need no more imply a general absorption than a white cloud in the summer sky implies a hazy atmosphere. Both Kapteyn and von Seeliger were however fully aware of the dangers to which their statistical studies of stellar distribution would be exposed if such absorption were appreciable. Not only would it have had the effect of making them overestimate the dimensions of the galaxy, but it might also have led them seriously astray in their conclusions as to the rate at which the numbers of the stars fell off at increasing distances from the sun. Thus we find them between 1904 and 1909 investigating the density which such an absorbing medium would have to have in order to produce an apparent falling off of the sort found, in a system in which the stars were actually uniformly spaced.

Now there is another effect besides the simple *dimming* of starlight which an absorbing medium might produce, and that is a *reddening* of starlight. We are familiar with two kinds of fog, the white fog and the red fog. The difference depends on the size of the particles composing the fog. If the particles are appreciably larger than a wave-length of light, all colours are absorbed equally, the fog is white and the street lamps are simply dimmed. But if the particles are slightly smaller than a wave-length of light the blue and violet parts of the spectrum suffer more absorption than the red and yellow parts. We then have a red fog, and the street lamps take on a more ruddy tinge the further away they are; for whereas most of their blue rays are absorbed a considerable proportion of their red rays still get through. The reddening effect is called " selective absorption " to distinguish it from simple dimming which is called " general absorption." Between 1909 and 1914 attempts were made by Kapteyn and several other investigators to detect a reddening of the more distant stars. Stars were selected in which the colours had been measured and in which the spectral types had been determined independently from the spectral lines. It was found that those stars, which had the slowest apparent motions across the sky (on the average the most distant stars), were on the whole definitely redder than the stars of the same spectral type, which showed more rapid motion and were therefore on the average nearer to us. Though the various investigators differed as to the amount of the absorption found, they agreed in the main that it was considerable.

It now seems pretty certain that these early results were spurious, and due largely to a phenomenon which was not appreciated at the time. It is now realized that giant stars are slightly redder than dwarf stars of the same spectral type. And unless one takes precautions, not then available, for distinguishing these two types of stars, a haphazard assortment of the more distant stars will contain a larger proportion of bright giants than will a similar assortment of the nearer stars.

It was only in 1915 that we began to get any answers to this very vital question which were at all convincing. The lead was taken by Professor Shapley who was then working at Mount Wilson. He used for the purpose the globular clusters and he used them in three different ways. First he investigated them for evidence of selective absorption, or reddening-effect. He began by measuring photographically the colours of individual stars in the globular cluster in Hercules, which is the brightest in the northern hemisphere; and since he found many which were as white as the whitest stars known, and showed not the slightest tinge of yellow, he was able to conclude that in this direction at least selective absorption was negligible. Examinations of other and more remote clusters quickly followed and led him to essentially the same result. Yet even though selective absorption had no appreciable effect on the light of the globular clusters it was still possible that there might be an effect due to general absorption.

Having previously shown that the actual dimensions of the globular clusters differed but little, and that the variety in their apparent sizes was due to differences in distance, Professor Shapley proceeded to measure their apparent brightnesses and to correlate them with their apparent diameters. Had there been any appreciable dimming of light in space, the brightnesses would have fallen off more rapidly than the diameters, and the more distant clusters would have appeared larger in proportion to their brightness than the nearer ones. But again no such effect was found.

Finally, Professor Shapley used these clusters to investigate a third phenomenon which an absorbing medium might be expected to show. When light traverses such a medium the rays at the blue end of the spectrum tend to be slowed up more than those at the red end. In a star shining with a steady light nothing abnormal will be noticed; but if a star is a variable its time of maximum, observed in blue light, should be delayed longer than its maximum observed in red light. The relative delay—the interval between the red and blue maxima—will obviously increase with the distance of the star. In 1917 the times of maxima in red and blue light were carefully compared for eleven Cepheid variables in a cluster in Serpens. But though the rays had taken some tens of thousands of years over the journey Professor Shapley was able to show that the red rays had certainly not gained as much as a minute on the blue rays—a most powerful argument against absorption.

The next step, which was to make use of the still vaster distances traversed by light reaching us from the spiral nebulae, was taken by Professor Lundmark and by Dr. Lindblad between 1917 and

1920 in a similar research on the Andromeda Nebula and others of the nearer spirals. Finally in 1926 Professor Shapley and the late Miss Ames used the same methods in the study of 300 spiral nebulae in the Virgo super-galaxy which lies at a distance of no less than ten million light-years. Again the results were all practically negative; and the failure to detect absorption over distances comparable to the greatest known in astronomy seemed to give reasonable assurance that it might be safely neglected. It appeared that the warning note sounded by Barnard more than twenty years earlier had been, in fact, a false alarm.

It was, however, just about this time that it was becoming increasingly obvious that the spiral nebulae were external galaxies on a par with our own. But one serious objection was that our own galaxy appeared to be very much larger than any of the others. There was of course no vital reason why it should not be so. Yet one of the most attractive points in the theory of external galaxies was that it put another nail into the coffin of a geocentric universe; and, if our own galaxy was to turn out to be built on a scale so much superior to that of any of the others, the effect of driving the nail in would be largely spoilt. From this contingency there was one possible escape. The negative results on absorption had all been obtained from studies of the globular clusters and spiral nebulae; and as we have seen these bodies avoid like the plague the neighbourhood of the Milky Way. Thus although there could be little or no absorption in directions *away* from the Milky Way, there might still be considerable absorption *in* the direction of the Milky Way. Moreover if such *galactic* absorption, while extending through the length and breadth of the galaxy, was confined to its plane as a shallow layer, our estimates of the distances of bodies above and below the layer would be scarcely affected, while those of the diameter of our own galaxy might be grossly exaggerated. In other words the detection of galactic as opposed to intergalactic absorption might conceivably bring about an equalization of our own with the external galaxies.

Nevertheless the general belief in the non-existence of any widespread galactic absorption continued to grow stronger, until in 1930 it was rudely shaken by a discovery of Dr. Trumpler at the Lick Observatory. From the spectral types, colours and apparent brightnesses of the component stars, he had determined the distances and actual dimensions of 100 of the open star clusters in the Milky Way. The fantastic result had emerged that the open clusters increased regularly in their actual sizes with increasing distance from the sun. Had it turned out that the clusters of different sizes were arranged quite haphazardly as regards distance, or even that the clusters were all of the same size, it would not have been surprising; but the arrangement that was found was much too reminiscent of the Adventures of Alice in Wonderland to be interpreted literally. The obvious explanation was that there was considerable absorption in the direction of the Milky Way which made the more distant clusters appear further off and therefore larger than they really were. Dr. Trumpler showed that on the simple assumption of a shallow layer of mistiness extending with a certain uniform density through the length and breadth of the galaxy, the corrected estimates of the dimensions of the clusters would come out closely the same for them all irrespective of their distances. He found that the mist produced about equal amounts of general and of selective absorption and would just about halve the photographic brightness of a star 3,000 light-years distant. On the other hand the layer was about 600 to 900 light-years deep and thus extended to about 400 light-years only above and below the central plane of the Milky Way. This explained why the globular clusters and spiral nebulae which are mainly situated well above or below this plane had given no evidence of absorption.

Dr. Trumpler's discovery was the signal for a renewed attack on the whole problem. During the next three years Dr. van de Kamp of the Leander McCormick Observatory, Charlottesville, Virginia, and Dr. Hubble again made use of the globular clusters and spiral nebulae; but this time it was not the colour or brightness of these bodies which was considered but their distribution in the sky. The manner in which the globular clusters congregated in two belts on either side of the Milky Way and scrupulously avoided its immediate vicinity had usually been regarded as an effect of real spatial distribution: there was possibly some influence at work that prevented the globular clusters from entering this so-called "zone of avoidance." Dr. van de Kamp, however, was able to show that the zone of avoidance and the way in which the number of the globular clusters fell off as it was approached, was perfectly compatible with a shallow layer of galactic mist of similar density and distribution to that deduced by Dr. Trumpler. In other words the globular clusters were not found near the Milky Way simply because the galactic mist blotted them out. The extra-galactic arrangement of the spiral nebulae had likewise been previously regarded as a real effect of spatial distribution. In their case the essential point had seemed to be not so much their avoidance of the Milky Way as their concentration towards the two regions of the sky furthest from it—the so-called north and south galactic poles. In the days when it was thought that the spiral nebulae were off-shoots of our own stellar system, it had been vaguely suggested that this polar distribution was the result of their repulsion by some unknown force acting at right angles to the galactic plane. And subsequently when they came to be looked upon as external galaxies the polar distribution was taken as another example of their well-known gregarious tendencies, it being supposed that our own galaxy was situated between two super systems of galaxies which just happened to lie in the directions of the galactic poles. But so far the data upon which our knowledge of the distribution of the spiral nebulae was based had been somewhat incomplete. The earlier surveys had been limited to the brighter spirals, while the subsequent surveys of the fainter and more numerous spirals had covered only a limited number of sample regions of the sky. The survey which Dr. Hubble had now completed and which he published in 1934 was undoubtedly the most exhaustive that had been made hitherto, being based on counts of no less than 80,000 spirals photographed with the Mount Wilson reflectors. From this it appeared that, although the number of the spirals fell off gradually in all directions from the galactic poles, the rate at which the number diminished was much slower than had been thought. On the other hand the falling off in the numbers speeded up very markedly in the immediate vicinity of the Milky Way. So after all it was a galactic avoidance rather than a polar concentration that the spirals exhibited. Dr. Hubble concluded that apart from their tendency to cluster together in groups the spirals were in general distributed uniformly over the whole sky; and that their avoidance of the Milky Way was apparent only, and due to absorption by a shallow layer of galactic mist similar to that deduced independently both by Dr. Trumpler and Dr. van de Kamp.

A good illustration of the phenomenon suggested is provided by those low-lying mists one sometimes experiences on fairly high ground. The mist which may be quite dense and extensive may yet reach only slightly above one's head. Lights near to the ground may only be visible to a distance of a few yards while overhead the stars may be shining practically undimmed. As we lower our eyes towards the horizon the number and brightness of the stars fall off at first very slowly and then, quite suddenly, much more quickly; while in the immediate vicinity of the horizon the stars are completely blotted out.

In view of the overwhelming evidence that had now accumulated in favour of galactic absorption, Professor Stebbins, Director of the

Washburn Observatory, Madison, Wisconsin, and one of the pioneers in the application to astronomy of the photo-electric cell, visited Mount Wilson in 1933 to see whether it was possible, with this delicate instrument attached to the 100-inch telescope, to detect after all a reddening of the globular clusters nearest to the Milky Way. It was true that nearly twenty years earlier Professor Shapley had failed to find any such reddening; but he after all had been interested in an absorption that was universal rather than galactic and had concentrated on the clusters that were most remote. Professor Stebbins on the other hand was on the look-out for pure galactic absorption and therefore concentrated on the clusters that were closest to the Milky Way; moreover his photo-electric method was more sensitive in detecting colour difference than Professor Shapley's photographic method. And so it was not surprising that Professor Stebbins' trip to California gave him the results he anticipated: for it was found that there was an appreciable reddening effect in the clusters nearest to the zone of avoidance, which diminished rapidly and became practically negligible in clusters that were well clear of the zone.

The question of galactic absorption thus appears to be settled in principle. Four separate investigations point to the existence of an extensive but shallow layer of absorbing material, and they agree in the main as to its density and depth; on the other hand outside this shallow galactic layer absorption is negligible. We next ask what effect on our distance estimates will the recognition of this absorbing layer have? As far as those spiral nebulae and globular clusters that are well clear of the Milky Way are concerned the effect will be nil, as was originally shown by Professor Shapley, Professor Lundmark and others. But in the case of the stars and open clusters in the Milky Way and of the spiral nebulae and globular clusters close to its borders the effect may be considerable. Professor Stebbins finds that the globular clusters nearest to the zone of avoidance may have their light cut down to one-sixteenth of their original brightness and thus come out, according to calculations based on their measured brightness, four times as far away as they really are. But this " distance correcting factor " of four drops off very quickly with increasing distance from the galactic plane: thus it has dropped to two for clusters seen about five degrees above or below the plane, and has become practically negligible at slightly greater elevations. Using these corrected distances Professor Stebbins has replotted the distribution in space of the globular clusters and gets a result differing in important respects from that obtained by Professor Shapley in 1930. It will be remembered that Professor Shapley found the globular clusters to be arranged in a system shaped like a somewhat flattened bun. Its longest diameter, that of the buttered layer, was 180,000 light-years, its shortest diameter perpendicular to the buttered layer was 90,000 light-years ; while the solar system situated actually in the buttered layer lay at about 50,000 light-years from its centre. The distribution as modified by Professor Stebbins resembled a bun that was practically spherical. The diameter perpendicular to the buttered layer remained as before; the diameter of the buttered layer was reduced to the same figure, from 180,000 light-years to 90,000 light-years; while the solar system within the buttered layer came out at 30,000 light-years from its centre and 15,000 light-years from its edge. Again, as in Professor Shapley's model, these figures were derived purely from the distribution of the clusters, it being assumed that the diameter of the galactic layer itself was co-extensive with that of the enveloping system of clusters.

Another phenomenon that is closely allied to galactic absorption must now be briefly considered. In 1904 Dr. J. Hartmann of Buenos Ayres had noticed that in a bright B-type spectroscopic double star in Orion there were narrow lines of calcium which did not partake of the to and fro oscillations of the other spectral lines. He suggested therefore that the lines did not originate in the star itself but were produced by a cloud of calcium vapour lying between the star and the earth. Subsequently Dr. Slipher of the Lowell Observatory had found these same fixed lines in several other B-type spectroscopic doubles; while Miss Heger at Lick had shown that fixed lines of sodium were often present at the same time. It was however to Professor J. S. Plaskett, Director of the Dominion Observatory, Victoria, B.C., that we owe the first systematic study of these fixed spectral lines. In 1924 he showed that the positions of these lines differed from their normal positions in the spectrum by an amount and direction that closely corresponded to the velocity of the solar system through space. In stars in that part of the sky towards which the sun is moving the fixed lines would give a velocity of *approach* equal to the sun's velocity; in stars in the part of the sky away from which the sun was moving the lines would give a velocity of *recession* equal to the sun's velocity; while in stars in parts of the sky intermediate between these two points the lines would give intermediate velocities of approach or recession according to the ordinary laws of foreshortening. This indicates that the calcium and sodium clouds are not only outside the stars in whose spectra the lines are found, but are quite independent of these stars and lie more or less motionless in space. Actually the rule given above fails in certain parts of the sky to be exactly correct, so presumably portions of the cloud may be moving slowly in relation to the cloud as a whole.

The reason the fixed lines are generally found only in O and early B-type stars is that the atmospheres of these stars do not of themselves produce any appreciable calcium absorption. In stars of later type the lines of calcium produced by their own atmospheres conceal any lines that might originate in intervening clouds. Again in these very early type stars the lines proper to their own atmospheres are broad and diffuse and readily distinguished from the very narrow calcium lines of the clouds. When an O or B-type star happens to be a spectroscopic double, the fixed calcium lines are recognized still more readily by contrast with the oscillating lines of the stars themselves. There is however one other class of star in which the fixed lines of sodium and calcium are often detected. We saw in fact how in certain stages of a nova such lines stand out clearly; for whereas they are sharp and narrow and placed in their normal positions in the spectrum, the absorption lines proper to the Nova are broad and hazy and markedly displaced towards the violet.

In 1926 Dr. Otto Struve[1] working at the Yerkes Observatory, of which he has since been made director, confirmed the results of Professor J. S. Plaskett and showed that the fixed lines were most intense for stars nearest to the Milky Way and were usually absent or very faint in stars far from the Milky Way. There were, however, occasional exceptions, a star well removed from the Milky Way sometimes giving quite strong fixed lines. This indicated that whereas the calcium-sodium cloud was in the main closely confined to the plane of the galaxy there were here and there upshoots which extended to much greater distances from it.

By 1930 Professor J. S. Plaskett was able to go still further in his conclusions. In collaboration with Mr. Pearce he showed that the intensity of the fixed lines was in general proportional to the distances of the stars in whose spectra they appeared. In other words, the further off a star was the greater was the depth through the calcium cloud that its light had to pass before reaching us. Presumably therefore the cloud extended more or less uniformly through the length and breadth of the galactic system and thinned

[1] Dr. Otto Struve comes of an interesting line of famous astronomers. His father, Dr. Ludwig Struve, was director of the Karkov Observatory. His grandfather, Otto Struve, was director of the Pulkowa Observatory, as also was his great-grandfather, F. G. W. Struve.

off, as Dr. Struve had shown, above and below the galactic plane except for occasional and irregular upshoots of greater density. In consequence of this important relation between the intensity of the fixed lines and the distance of a star, it is possible to use the intensity of the fixed lines as an independent criterion of the distance of a nova, for the majority of these objects lie close to the galactic plane where the density of the cloud seems to be most uniform.

It might have reasonably been supposed that the sodium-calcium cloud was co-extensive if not identical with the galactic mist responsible for general and selective absorption; and that the study of the intensity of the fixed lines in various parts of the sky would provide an independent check on the distribution of the galactic absorption as determined by Drs. Trumpler, van de Kamp and Hubble and Professor Stebbins. But as far as the observations go at present this does not appear to be the case. Although there is some correlation between the intensity of the fixed lines on the one hand and the reddening and dimming effects on the other, it seems that the calcium-sodium cloud is less closely and uniformly confined to the vicinity of the galactic plane than the generally and selectively absorbing mist.

Another thing that our studies of galactic absorption and fixed spectral lines have taught us is that interstellar space is far from being empty. It is true that the cloud and the absorbing layer are almost infinitely rarefied, but on the other hand they are enormously extensive. And when we remember how vast in comparison with the stars themselves are the intervening spaces, we can see that the highly rarefied interstellar material may yet have a very great total weight. Professor J. S. Plaskett has in fact estimated that it contributes no less than forty per cent. of the total weight of our galaxy.

CHAPTER XIX

ROTATION OF THE GALAXY

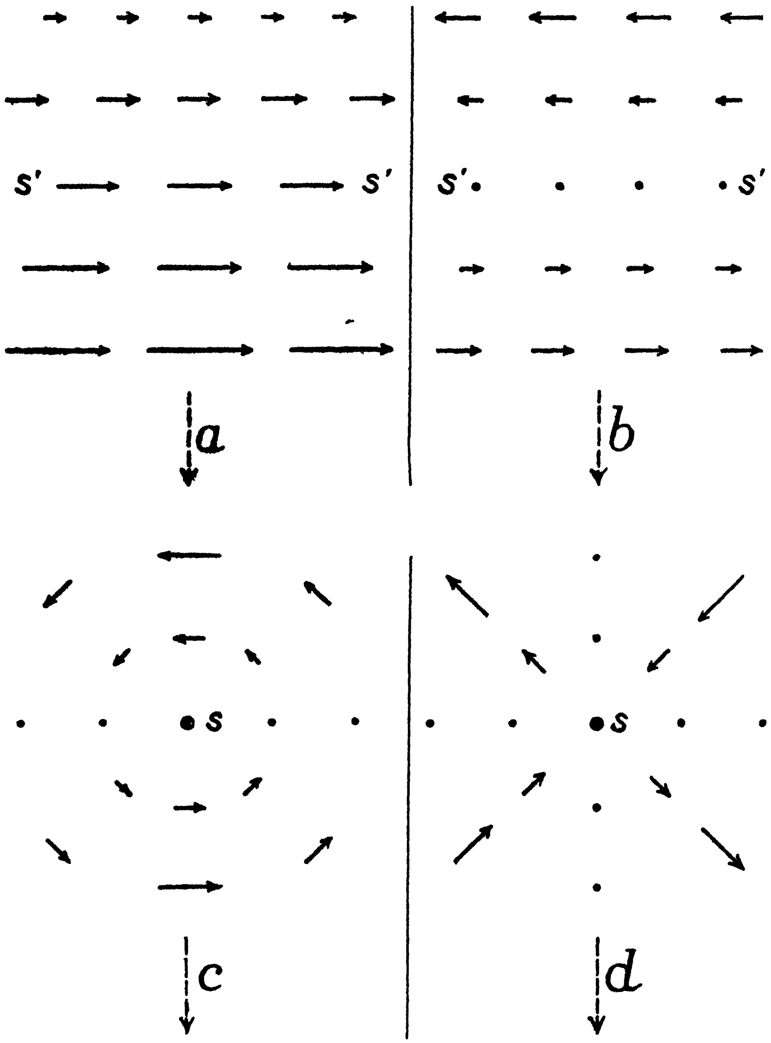
THE earliest suggestion that the galaxy is rotating about its centre seems to have been made by John Herschel. He considered its flattened disc-like shape, discovered by his father, to be evidence of such rotation, the rotation in fact being the cause of its flattening. Yet it was only twelve years ago, when Professor Lindblad and Dr. Oort tackled the problem, that any serious effort began to be made to consider the practical implications of galactic rotation and to attempt its measurement. As so often happens when a new problem is brought to the front there turns up, from some unexpected quarter, the record of a much older investigation which is found to foreshadow clearly what was believed to be a quite novel result. And so it was with the present problem; for just as the work of Professor Lindblad and Dr. Oort was beginning to bear fruit it was found that the Swedish astronomer, Gyldén, had discovered in 1871 a curious relation in regard to the motions of the stars which he suggested was the result of galactic rotation.

There are two ways in which the galaxy might *a priori* be supposed to rotate. Either it might rotate as though its component parts were imbedded in a rigid wheel, when its outer parts would travel more rapidly than its inner parts; or it might rotate in the same way as the solar system or the rings of Saturn, when its stars and star clusters would travel more rapidly the nearer they were to the galactic centre.

In the first case we should only be able to get evidence of the phenomenon from observations of such bodies as the spiral nebulae which lay outside the galaxy and did not share in its rotation; for obviously objects that are fixed to a rotating wheel have no motions relative to one another. Moreover, it was likely that the very large individual velocities which the spirals were known to possess would tend to obscure the probably much smaller apparent velocities impressed upon them by galactic rotation.

On the other hand if the galaxy is rotating like the solar system or the rings of Saturn, the sun will be lagging behind the stars that lie between it and the galactic centre, and travelling on ahead of those that are situated in the part of the sky opposite to the galactic centre. This differential effect of rotation—the slipping, so to speak, of contiguous zones of the galactic system one upon the other—ought to be shown up by observation. But observation can detect only two parts of a star's real motion through space: (1) its motion exactly towards or away from us, which is detected by the spectroscope and called radial motion; and (2) its motion perpendicular to the line joining us to the star (i.e., its motion across the background of the sky) which is determined by direct observation with a telescope and is called its proper motion. And so we must consider in what way the comparison of both the radial motions and proper motions of the stars in different directions in the sky might enable us to detect and measure, if present, a galactic rotation of the differential or Saturn's rings type.

The effects to be expected will be best understood by recourse to the diagram. In each of the four parts of the diagram the centre of the galaxy is supposed to be situated in a direction vertically downwards and at a considerable distance off the page. In (a) the arrows represent the actual velocities of individual stars revolving in five adjacent galactic zones round the distant galactic centre. The middle zone (S′ S′) is that in which the sun is, while the two upper zones are somewhat further from the galactic centre and the two lower zones are somewhat nearer the centre. The lengths of the arrows are proportional to the velocities of revolution and so are greatest in the lowest zones. In (b) the *absolute* velocities have been converted into velocities that are *relative* to the solar zone; hence the stars in the solar zone, which is now looked upon as stationary, become dots, while those in the upper zones which are lagging behind the sun have velocities directed backwards to the left, and those in the lower zones which are gaining on the sun have velocities directed forwards and to the right. In (c) we take the central dot (S) in the solar zone to represent the sun itself, and draw the arrows proportional in length to that part of the velocity of each star relative to the sun which is perpendicular to the line joining the star to the sun—proportional, in fact, to the velocity of its proper motion. It will be seen that the velocity of the proper motion is greatest in the two directions in the sky exactly towards and exactly away from the galactic centre, and falls off gradually to zero in the two perpendicular directions—the directions towards which and away from which the sun in its revolution is travelling. In (d) we again take the dot (S) to represent the sun, but this time we draw the arrows to represent that part of the velocity of each star relative to the sun which is directly towards or away from the sun—to wit the velocity of its radial motion. This last part of the diagram shows that for stars lying directly towards and away from the galactic centre the radial motion is zero; for obviously such stars are neither approaching nor receding from the sun. Again the radial motions are zero for stars situated in the perpendicular directions; for these are revolving in the same zone as the sun, with the same velocity and in the same direction. But in directions intermediate between what we may call the four *cardinal points*, the radial motions are no longer



zero and attain maximal values in four directions inclined to the cardinal points at forty-five degrees; in two of these directions the motion is towards the sun, in the other two it is away from the sun. Another point illustrated by the diagram is that both the radial and proper motions produced in any given direction by galactic rotation increase in velocity in proportion as the distance of the stars from the sun increases: thus in (c) and (d) the arrows get longer the further they are from the sun.

It follows that if there is any galactic rotation of the type exhibited by Saturn's rings we ought to find: (1) that the velocities of the proper motions of the stars have maximal values in *two* directions in the sky towards and away from the galactic centre, the values being opposite to one another in the two directions; (2) that the radial velocities of the stars should have maximal values in *four* directions in the sky inclined at forty-five degrees to the directions towards and away from the galactic centre (in two of these the velocity will be one of approach and in the other two one of recession); and (3) that the velocities of either kind thus found should be greater in proportion as the distances of the stars from the sun are greater.

Needless to say the above explanation has been stripped of all complications. There will of course be superimposed on any galactic rotation there may be, the individual motions of the stars. The only way to disentangle the galactic rotation from the idiosyncrasies of individual motions is to make an elaborate statistical study of the apparent motions of a large number of stars in order that the idiosyncrasies may cancel out.

About 1926 Professor Lindblad developed a general theory of galactic rotation which suggested an explanation of certain matters in connexion with stellar movements that had long puzzled astronomers. He supposed that our galaxy was made up of a number of sub-systems—e.g., the system of the open clusters, the system of the globular clusters, the system of the ordinary run of stars and the systems of the various types of variable stars, etc.—which partook in varying measure of the general tendency to revolve around the galactic centre. Theoretically the sub-systems endowed with the most rapid galactic revolution should revolve in orbits most nearly coincident with the galactic plane, and thus should exhibit the most marked galactic concentration. Their orbits also should be nearly circular; and their velocities at a given distance from the galactic centre should deviate but slightly from one another. On the other hand sub-systems endowed with slower speeds of revolution should move in orbits with a much larger range of inclinations to the galactic plane; so that their distribution about the galactic centre would be much less flattened or disc-like and much more nearly spherical. Moreover, their orbits ought often to be markedly elliptical; while the velocities of the individual members of the sub-system instead of being all nearly the same should extend over a large range of values.

On this theory the open clusters, the galactic nebulae and the great majority of the stars including the sun, which as we have seen show a very marked concentration towards the Milky Way, should be members of the sub-system moving with the highest rotational velocity. The observed fact that all these bodies have comparatively small velocities in space relative to the sun also fits in with the theoretical requirement that the members of such a sub-system should all revolve in nearly circular orbits with velocities confined within a narrow range. On the other hand objects like the long-period variable stars, the shortest period Cepheids, the globular clusters, etc., whose distribution in space is nearly spherical and only very slightly flattened towards the galactic plane, and which therefore must be revolving in orbits that are often steeply inclined to the galactic plane, should according to the theory be members of sub-systems moving with small rotational velocities. Yet oddly enough these are the very objects that are known as " high velocity bodies " because they are found to have abnormally large velocities relative to the sun. Now only the year before, in 1925, Dr. Strömberg at Mount Wilson had shown that if one considered the average motions of the various groups of " high velocity bodies " it turned out that each group as a whole had a very considerable velocity, and that although the velocities of the different groups differed greatly, *all the groups were travelling towards the same direction in the sky.* This curious phenomenon, known as " Strömberg's asymmetry " of stellar motions, was at once explicable on Professor Lindblad's rotational theory. The point in the sky towards which Dr. Strömberg had found the groups of high velocity stars to be travelling lay not only close to the galactic plane but also almost exactly at right angles to the direction of the galactic centre as derived by Professor Shapley from his study of the distribution of the globular clusters. One could thus regard the " high velocity stars " as belonging to sub-systems with abnormally *slow* velocities of revolution round the galactic centre: their observed high velocities relative to ourselves would then be due to our own sub-system continually overtaking them and leaving them behind—they were the laggards, in fact, on the galactic round-about.

We pointed out that theoretically sub-systems showing but little galactic concentration and revolving in orbits often of steep inclination should not only revolve more slowly but exhibit less uniformity in their individual velocities than sub-systems with marked galactic concentration. In other words, if the long period variables are really members of a slowly revolving sub-system, then the individual velocities of the long period variables should be scattered over a much wider range of velocities than the individual velocities of the stars belonging to the sub-system of the sun. Further, if we were to arrange the various sub-systems of long period variables, Cepheids, globular clusters, etc., in order of *diminishing* velocity of revolution round the galactic centre, we ought to find that the order is that in which the *range*, over which the individual velocities of each group are scattered, progressively *increases*. Actually this very phenomenon had already been pointed out by Dr. Strömberg in his work on the asymmetry of stellar motions. He had emphasized as a puzzling and unexplained fact that in the various groups of high velocity stars the faster the group as a whole was travelling the greater was the " scatter " in the velocities of the individual members of that group. Take for instance four well-defined high velocity groups: (1) the long period variables with periods *exceeding* 210 days, (2) the Cepheid variables with periods less than 17 hours, (3) the long period variables with periods *less* than 210 days, and (4) the globular clusters. It is found that the velocities of these four groups relative to the sun are respectively 40, 70, 100, and 170 miles per second. On the other hand what we may roughly regard as the average velocity of the individuals in each of these groups relative to the corresponding group as a whole—more correctly the *dispersion* of the individual velocities about the average—is 30 miles per second for the first, 45 miles per second for the second, 55 miles per second for the third and 70 miles per second for the fourth group. In other words the velocity of a group as a whole increases progressively as the random velocities of its individual members increase. And since Dr. Strömberg's criteria of *increasing* velocity relative to the sun are identical with Professor Lindblad's criteria of *diminishing* velocity of revolution, it follows that Dr. Strömberg's observational data are in accordance with the requirements of the theory of galactic rotation: that the random individual velocities increase as the velocity of revolution of a sub-system decreases.

Following on the development of these ideas by Professor Lindblad came the work in 1927 of Dr. Oort of the Leiden Observatory who tackled the matter from a more directly practical point of view. He deduced those effects on the proper motions and radial velocities of the stars which galactic rotation should have, and which we explained above with the aid of our diagram; he expressed those effects in precise mathematical form; and he looked for, and succeeded in discovering, them in the measurements of radial velocities that were available at the time. It will be remembered that as a result of a galactic rotation of the Saturn's Rings type, the radial velocities of the stars should have maximal values in four directions in the sky oriented diagonally in respect of the direction towards and away from the galactic centre; that the radial velocities should be towards us in two of these directions and away from us in the other two. Dr. Oort's analysis of the available radial velocity measurements pointed clearly to such an effect and thus enabled him to indicate (1) the direction in the sky towards which the galactic centre lay, and (2) the direction in the sky (at right angles to the above) towards which the galactic rotation was carrying the sun. His results were most encouraging. The direction which his radial velocities gave him for the galactic centre was almost identical with that which Professor Shapley had found for the centre of the system of the globular clusters and for the densest part of the Milky Way star clouds. Again, the direction towards which his radial velocities indicated that the sun was revolving was almost exactly the same as that which Dr. Strömberg's high velocity stars were moving away from. Thus Dr. Oort's analysis confirmed not only the direction of the galactic centre as determined by the quite independent methods of Professor Shapley, but also the correctness of the interpretation put upon Strömberg's asymmetry by Professor Lindblad: that the "high velocity stars" were really members of sluggish sub-systems lagging behind the rapidly revolving sub-system of the sun. In addition Dr. Oort found that in the four directions in which the radial velocities were greatest the radial velocities were larger in proportion as the distances of the stars from the sun were larger—which it will be remembered is another effect produced by galactic rotation. Altogether therefore the evidence was strongly in favour of Dr. Oort's being on the right track.

It was about this period that the investigation we have referred to by Gyldén (published in Swedish in 1871) was rescued from obscurity. From a study of the proper motions of the stars available at that time he discovered the effect illustrated in the part c of the diagram: that in one part of the sky the stars tended to drift in one direction, while in the opposite part of the sky they tended to drift in the opposite direction; and towards directions at right angles to these two, the tendency to drift gradually petered out. Considering the inaccuracy of those proper motions, the scarcity of them, and the complete lack of them in much of the southern hemisphere, it is an amazing tribute to his skill that he succeeded in detecting the effect at all. Yet he not only discovered the effect, but he explained it in the way we have done as due to galactic rotation, and found for the line towards and away from the galactic centre a direction very close to that now accepted. Further, in order to show that his method for determining the direction of the galactic centre was a sound one, he hit upon the ingenious idea of working out the direction of the centre of the solar system—to wit, that of the sun—from the apparent motions on March 21, 1868, of the minor planets. And the answer he got was not far from the truth.

The results of Dr. Oort were beautifully confirmed by the discussion, by Professor J. S. Plaskett and Mr. Pearce, of the radial velocities of O and B-type stars observed at the Dominion Observatory, Victoria, B.C. These stars were particularly well suited for the purpose: their great absolute brightness enabled them to be observed at enormous distances, their marked galactic concentration placed them in those directions in which the rotational effects were best looked for, and their very small individual motions did not tend to obscure those motions due to galactic rotation which were required. Since these are the stars which show in their spectra the fixed calcium lines, the authors were able to use the intensities of these lines, in the way we have previously explained, as an indication of the relative distances of the stars. And as was to be expected, it was found that the effects of galactic rotation on the radial velocities increased in proportion to the stars' distances. But most interesting of all was the fact that the fixed lines also showed the rotational effect, and moreover an effect almost exactly *half* of that given by the ordinary spectral lines of the stars. If the interstellar calcium cloud extends uniformly throughout the galactic plane, then the *average* distance from us of that part of the cloud which lies between us and a given star will be *equal to half* the distance of the star. If the cloud is sharing in the rotation of the galaxy, and its radial velocity increases in proportion to its distance from us, then the observed radial velocity of a particular stretch of it lying between us and a given star will be the *average* radial velocity of that stretch, to wit, the radial velocity of the cloud at a point *half-way* from us to the star. Thus were three problems solved at one time: the rotation of the galaxy was confirmed; the calcium cloud was shown to share in that rotation; and additional evidence was secured that the cloud extended more or less uniformly throughout the length and breadth of the galactic plane.

Though there could no longer be much doubt that observation had proved the existence of the expected rotational effects, it was still possible that the observed effects were due to a local rotation rather than to a rotation of the galaxy as a whole. Even the observations of the O and B-type stars had only traced these effects to a distance of about 5,000 light-years from the sun, which was small compared with the distance from the sun to the centre of the system, now put at 30,000 light-years but then supposed to be much larger. This criticism has been met to some extent by Dr. Joy's study at Mount Wilson of the radial velocities of 124 distant Cepheid variables. In them the rotational effect was definitely present, but it was much smaller than it ought to have been for such distant stars. A group of the remotest of these Cepheids, about 20,000 light-years distant gave a rotational effect in their radial velocities no larger than ought to have been given by stars at half that distance. The discrepancy however was cleared up as soon as due allowance was made for galactic absorption, for the necessary correction about halved the distance of these particular stars.

So far the recent investigations we have referred to have all made use of the radial velocities of the stars. The reason is that the effects of galactic rotation on the proper motions is much less marked. It is true that the effect on the velocity of the proper motion in miles per second increases in proportion to the distance of the star—just like the effect on the radial velocities; but that unfortunately we cannot observe. As observed, the transverse motion of a star across the sky is an *apparent* motion and is measured as an angle; and the apparent motion of a star travelling at a given number of miles per second *diminishes* in proportion as the distance of the star *increases*. Consequently since an increase in distance tends to *increase* the effect due to galactic rotation for one reason, and to *diminish* it for another, and to do both in the *same* proportion, the net result will be no change. In other words, the rotational effects on the observed proper motions will be quantitatively the same at all distances, though at greater distances they will be less liable to be obscured by the random, individual motions of the stars. Nevertheless it was a matter of interest to see what confirmation of the radial velocity results a study of the proper motions might give, and this was carried out in 1929 by Sir

Frank Dyson. Considering the comparative inaccuracy of the method, the results were in satisfactory agreement with the rotational theory; and although the rate at which the effect was found to increase with the distance was unexpectedly small, the direction obtained for the galactic centre was the same as that given by the radial velocities.

So far the observations have given us three pieces of information: (1) the direction of the galactic centre, (2) the direction, at right angles to it, towards which the sun with its particular zone is revolving, and (3) the velocity of revolution of the solar zone *relative* to the various galactic zones lying at different distances on either side of it. We have as yet no information as to the actual velocities with which the solar zone or any other zone is travelling round the galactic centre. Fortunately we ought to be able to get an approximate idea of the sun's actual velocity of revolution from the globular clusters. We saw that the globular clusters were distributed spherically about the galactic centre and could not therefore, as a group, partake of the general rotation of the galaxy about its central axis. Individually the clusters may be moving rapidly, but their steeply inclined and highly elongated orbits will carry them more or less directly towards and away from the galactic centre. They may be likened to a swarm of gnats: the individuals are continually darting to and fro between the centre and the surface of the swarm, but the swarm itself has no tendency to rotate as a ball. If therefore the sun was stationary in the galaxy and not partaking of its rotation, the radial velocities of the globular clusters in one direction would exactly cancel out with those in the opposite direction. We saw, however, that according to Dr. Strömberg the radial velocities so far from cancelling out indicated for the globular clusters as a group a velocity towards a certain direction in the sky of 170 miles a second. This therefore we may regard, at least provisionally, as representing the velocity with which the sun is revolving round the galactic centre. Meanwhile is there any other method of checking up on this important matter?

Theoretically the ideal method for finding the velocity of the sun in its revolution round the galaxy is to measure the radial velocities of objects *outside* the galaxy, such as the spiral nebulae. For instance, if we wished to measure the velocity with which we were revolving on a roundabout at a fair, we could not do better than arm ourselves with a spectroscope and measure the maximum radial velocity of the " neon-sign " on some neighbouring building as we alternately approached it and receded from it. The only objection to the method is that unless the sun's revolution is fairly rapid the large individual velocities of the spiral nebulae will probably obscure it. If however our provisional estimate of 170 miles a second is anywhere near right, it certainly ought to be measureable. This investigation was carried out a few years ago by Dr. Hubble using the radial velocities of some of the nearer spirals measured at Mount Wilson. He found that the sun was moving relative to the system of the spiral nebulae at a speed of about 175 miles a second in the same direction as indicated by the globular clusters. Such good agreement between two quite independent methods can only mean that the result is fairly near the truth.

We now know two things about the speed with which the galaxy is rotating: Dr. Hubble and Dr. Strömberg have given us the actual velocity with which the solar zone is revolving; while Dr. Oort, Professor Plaskett and Mr. Pearce have given us the rate at which the velocities change for zones lying at increasing distances on either side of the solar zone. These two data in conjunction with Kepler's law enable us to calculate (1) the distance of the sun from the galactic centre, and (2) the total weight (or more correctly, mass) of the galaxy. Both answers will depend to some extent on how we assume the material composing the galaxy to be distributed —whether it is mainly concentrated near the galactic centre, or whether it is more evenly distributed. On the generally accepted assumption that it is mostly concentrated near the centre, the distance of the sun from the centre comes out at about 30,000 light-years, and the total mass of the galaxy is found to be about equal to that of 150 thousand million suns. Should the galactic material be more evenly distributed than is usually supposed, both these figures would have to be somewhat reduced.

We notice at once that the distance of the sun from the galactic centre obtained in this way is in agreement with that previously got from Professor Shapley's distances of the globular clusters after correcting for galactic absorption. Again, the remarkable convergence of two independent methods encourages us in the hope that the distance is at least of the right order of magnitude.

It is interesting too to compare the figure for the total weight of the galaxy with estimates made from actual star counts. The chief authorities on this subject are Dr. Seares at Mount Wilson and Professor van Rhijn at Groningen. By counting in sample regions all over the sky the numbers of stars between various limits of brightness, and by taking into consideration the rate at which the number of stars in a given volume of space begins to fall off at great distances, they conclude that the total number of stars in the galaxy is of the order of thirty thousand million. On the average a star has about half the weight of the sun; which makes the weight of all the stars in the galaxy equal to that of about fifteen thousand million suns, or about one-tenth of the total weight given by the rotational theory. But when we take into account the uncertainties necessarily attaching to the argument based on star counts, and remember that we have not yet included the weight of the nebulae and of the widespread absorbing clouds—which would probably about double the weight—the discrepancy appears less serious. Moreover, we saw that the weight deduced from the rotational theory would have to be reduced if the mass of the galaxy is less concentrated at its centre.

It is found that the time necessary for the sun to make one complete circuit of the galaxy is about 250 million years. Although this figure can be got from the values given above for the velocity with which the sun is revolving and for the distance of the sun from the centre, it can also be determined independently. It can be calculated directly from the differential effect alone: from the rate at which the velocities of revolution of the stars relative to that of the sun increase with increasing distance from the sun—to wit, the effect measured by Dr. Oort and others and known with considerable accuracy. The period of the sun's revolution is thus not subject to the uncertainty attaching to its velocity of revolution or to its distance from the galactic centre. It follows that within geological times the solar system has made some five or six complete revolutions of the galaxy. Also, depending on whether we adopt the shorter "time scale" of a hundred thousand million years given by the expanding universe theory, or the longer "time scale" of ten million million years given by the observed degree of equipartition of energy so far achieved, the number of galactic revolutions made by the solar system since the beginning of things will be respectively either 400 or 40,000.

It must be emphasized that the various figures we have deduced from the rotational theory and from the observations supporting it are in no sense to be taken as final. No one will be surprised if they have to be profoundly modified in the next few years. We can merely regard them as giving general indications, and be thankful that they diverge so little from the results derived by independent methods.

But the broad fact of galactic rotation is undoubtedly established both by direct and indirect evidence; and in order to make this quite clear we shall refer briefly to three additional arguments that strongly support it. First, in the case of the nearer spiral nebulae it has been shown spectroscopically that each is rotating about its centre with a velocity which decreases outwards from the centre, and is of the same order of magnitude as the rotational velocity found in our galaxy. Since the galaxy is almost certainly a spiral nebula it is equally certain that it is rotating like a spiral nebula. Secondly, the discovery of galactic rotation has at last explained why the otherwise universal rule that all the spirals are receding from the galaxy appeared to break down badly in the case of some of the nearer spirals. It is found that if we take into account the high rotational velocity of the sun and refer the motions of the spirals to the galactic centre instead of to the sun, practically all the velocities become recessional and the rule holds good almost universally. Thirdly, as Sir Arthur Eddington has pointed out, the existence of the calcium cloud and of the galactic absorbing material would be impossible if the galaxy were not rotating; for unless the gravitational attraction towards the galactic centre was neutralized by the centrifugal force engendered by the rotation of the diffuse material, that material would long ago have fallen in and collected at the galactic centre.

To summarize we may draw the following picture of the rotating galaxy, which while it gives a fair representation of the present state of our knowledge is only to be regarded as provisional. The galaxy is made up of sub-systems showing varying degrees of galactic concentration. Members of sub-systems showing the most marked galactic concentration revolve round the galactic centre in orbits that are closely confined to the galactic plane and are probably nearly circular. Members of sub-systems showing less marked galactic concentration revolve in orbits that are often steeply inclined to the galactic plane and are probably often highly elongated. In each sub-system the members that are nearest to the galactic centre revolve more rapidly than those further from the centre; and since those that are nearest the centre are also travelling in small orbits their periods of revolution round the centre are considerably shorter than those of members situated at greater distances from the centre. At any given distance from the centre the velocity of revolution is greatest for those sub-systems that are most closely confined to the galactic plane and least for those whose orbits show the largest variety in their inclinations to the galactic plane. Finally, in the rapidly revolving sub-systems with marked galactic concentration, the motions of individual members lying at the same distance from the centre are very uniform and differ but little from one another in speed and direction; on the other hand in slowly revolving sub-systems, showing but slight galactic concentration, the motions of individual members at the same distance from the centre may diverge widely from one another both in speed and direction.

The sub-system which has by far the largest membership is that with the most rapid rotation and with the most marked galactic concentration, the sub-system to which the sun belongs. It comprises the open clusters of the Milky Way, the galactic nebulae, the galactic absorbing material, the calcium cloud, and the great majority of stars of all spectral types. The other sub-systems, even when lumped together, have a very much smaller membership. These are the sub-systems of the various " high velocity bodies," sluggishly rotating sub-systems with a much smaller or even negligible degree of galactic concentration: the sub-system of the globular clusters, the sub-system of the short period Cepheids, the two sub-systems of long period variables, and sub-systems of a small minority of the stars—to wit, the " high velocity stars "—of the various spectral types.

CHAPTER XX

THE SUN'S MOTION THROUGH SPACE

HAVING dealt with the rotation of the galaxy, which represents the motions of the sun and stars on the grand scale, we return to consider again the much older problem of the *individual* motions of the sun and stars, their motions on the small scale. It may seem odd that the effects of the large scale phenomenon should be so much less obvious than those of the small scale phenomenon. But whereas we all take part in the first and so can only observe its small differential effects, we are able to observe directly the full effects of the second. That is why the individual motions of the stars were detected over two hundred years ago and the individual motion of the sun over a hundred and fifty years ago, while it is barely twelve years ago that the rotation of the galaxy was discovered.

We shall start with the individual motion of the sun. In Chapter VII we referred to the work on this problem up till 1901 when Professor Campbell made the first determination of the sun's motion from the *radial velocities* of the stars and got a result in fair agreement with the previous determinations that had always been made from *proper motions*. It will be remembered that in consequence of the bodily translation of the sun and solar system through space the stars as a whole appear to drift backwards in the sky—just as trees and hedges seen from the window of a railway carriage appear to move backwards in the opposite direction to that in which the train is travelling. It is this backward drift of the stars, the reflection of the sun's forward march, that we have to measure in order to get the direction and speed of the sun's motion. Now just as horses seen from the carriage window will have their own individual movements superposed on the backward motion which they share with the rest of the landscape, so will the individual motions of the stars be superposed on that backward drift which is common to them all. But whereas the solid ground provides a standard that at once enables us to say how much of the apparent motion of a horse is due to the motion of the train and how much to its own motion, there is no fixed standard in space that enables us to distinguish between the individual motion of a star and its backward drift due to the sun's motion. The best we can do is to determine the average motion of the *stars as a whole* relative to the sun, or—what comes to the same thing—the motion of the sun relative to the average of the *stars as a whole*. It is as though the stars were a swarm of bees in which the individuals were flying about at random while the swarm as a whole remained stationary or moved steadily on in a certain direction. We could then determine the motion of one particular bee (our sun) in relation to the average of all the other bees, in other words, its motion relative to the centre of the swarm.

The question that naturally arises at this juncture is "what exactly do we mean by the '*stars as a whole*'? Do we, for example, intend to include in our swarm all the stars in the galaxy? Clearly we do not; because the motion of the sun relative to the swarm that consists of all the stars in the galaxy is obviously its motion of revolution about the galactic centre—and that is the problem we have already dealt with. No, what we now require is the motion of the sun relative to the average of the stars that are *sharing* in its galactic rotation. Thus our swarm is to be limited not only to the stars that belong to the sun's own galactic sub-system, but further to such of them as are comparatively close to the sun and have approximately the same speed of galactic rotation as he has. We shall see presently that this definition of the standard to which the sun's motion is to be referred is in practice by no means water-tight. Nevertheless for convenience we shall accept it without further question for the time being.

Thirty years ago the problem of defining the standard to which the sun's motion should be referred was much less complicated than now; for we then knew nothing about the rotation of the galaxy, or about the existence of the various sluggishly revolving sub-systems of the "high velocity bodies." In those days it sufficed to define this standard as the average of all the stars—or rather of all the stars for which the necessary proper motions and radial velocities were available. It just happens, however, for a number of reasons that this older definition approximates fairly closely to the one we have given above. First, in determining the solar motion from the proper motions, only those stars could be used in which the proper motions were large enough to be detected; and this meant that in general it was only the nearer stars that were made use of. Secondly, in the determinations from the radial velocities only those stars could be used that were bright enough for their spectra to be photographed on a fairly large scale; and again, on the whole it is the nearer stars that are the brighter. Thirdly, it was soon discovered that the radial velocities of the vast majority of the stars did not exceed ten miles a second; and it seemed only reasonable in an investigation that was essentially concerned with the *general run* of the stars to exclude from the data employed those very rare stars with large radial velocities which were presumably exceptional bodies. It thus came about that, though it was not realized at the time, the data used in the earlier determinations of the sun's motion were automatically limited to a large extent to the nearer stars of our own galactic sub-system, mostly within a thousand light-years of the sun.

Although Professor Campbell's 1901 determination from the radial velocities agreed more or less with the previous determinations from proper motions in putting the point in the sky towards which the sun was moving somewhat south of the bright star Vega, his position was very definitely further south than that given by the proper motions. But as he had only 280 radial velocities to go on, and as very few of these were in the southern half of the heavens, the divergence of the two results had not been surprising. In 1910 and 1911 two new determinations were published which surpassed in reliability all previous ones. The first was that of Lewis Boss from an analysis of the proper motions of 5,972 stars given in his *Preliminary General Catalogue*. The other was a second determination by Professor Campbell based on the analysis of 1,193 radial velocities measured mostly at Lick. This time the divergence was considerably reduced, mainly by the northward movement of the point indicated by the radial velocities. But the divergence was still there; and on the whole astronomers tended to put their faith in the proper motions, and to suppose that as the data became more widely distributed over the heavens the radial velocities would gradually fall into line. Yet this did not happen. And during the next fifteen years further determinations from the radial velocities, by Professor Campbell at Lick, by Dr. Paraskevopoulos at Harvard, and by Dr. Gyllenberg and Dr. Malmquist in Sweden, all pointed more and more emphatically to the direction of the sun's motion being well south of the direction given by the proper motions.

It was not until 1925 that the cause of the trouble was discovered. It was then shown by Dr. H. Raymond and Dr. R. E. Wilson of the Dudley Observatory, Albany, that there were certain minute systematic errors in Lewis Boss' *Preliminary General Catalogue*, which when eliminated would shift the direction of the solar motion, as found from the proper motions, well south and bring it into close agreement with the direction found from the radial velocities. It will be remembered that in the preparation of his *Preliminary General Catalogue*, Lewis Boss, a previous director of the Dudley Observatory, had used the results of some seventy different catalogues of star positions. His main problem had been to discover and correct for the systematic errors in these various catalogues, due to the differences in the instruments, the observers, the methods, the climates, the localities, etc., in order to get a homogeneous mass of data, which would make the proper motions of the stars in all parts of the sky directly comparable with one another. Yet although he had succeeded in his purpose to a very high degree of accuracy, there had apparently remained over a minute systematic error sufficient to account for that curious discrepancy in the solar motion according as it was derived from proper motions or radial velocities.

Since 1925 the determinations of the solar motion by the two methods and by different investigators have on the whole achieved very satisfactory agreement; and we may now assume that relative to the nearer stars of our own sub-system taken as a whole, the point towards which the sun is moving lies somewhere within a certain area of the sky two or three times the apparent area of the full moon. We also know that its velocity, which can be derived only from the radial velocities of the stars, is very close to fifteen miles a second.

But all the time that the determinations of the solar motion had been gradually settling down and converging more and more upon a definite result, a secondary complication had been emerging which showed that the problem was not quite so simple as we have so far supposed. In the investigations of 1910 and 1911 it had been noticed both by Boss and Professor Campbell that the solar motion appeared to depend to some extent on the spectral type of the stars used for determining it. Thus in addition to using stars of all spectral types indiscriminately for a single determination, they also arranged the stars in groups according to their spectral type and made for each such group a separate determination. When this was done it was found that the sun's velocity through space was definitely less when judged by the yellowish-white stars than it was when judged by the whitest or the reddest stars. And the same thing applied to the direction of the sun's motion; the stars of intermediate spectral type indicated a point in the sky definitely further south than did either the earlier or later type stars. The extensive work of the last twenty-five years has amply confirmed this dependence of the sun's motion upon the spectral type of the stars used in determining it. Undoubtedly as one progresses from the whitest stars to the reddest the solar motion tends to lessen and shift southwards at first, and then as one reaches the end of the colour sequence it speeds up and shifts northwards again. Yet even this does not bring us to the end of the matter. For with increasing accuracy in the observations it became apparent that, though the spectral type may be the main factor, it is not the only factor on which the sun's motion depends. Apparently the deeper we probe into the mystery of the sun's motion the more the plot thickens. One might have thought that this was a most unsatisfactory and discouraging conclusion to reach; yet actually it is not so surprising and, up to a point, its significance is clear. It simply means that in the swarm comprising the nearer members of our own galactic sub-system the motions of the stars are not after all entirely at random. For some reason stars of the same spectral type have in addition to their individual motions a motion relative to the main swarm which they share in common, and this group motion differs slightly from one spectral group to another. It is as though we were considering the shipping from England to North America: we should find that the majority of boats, of certain definite colour-schemes, were sailing in the direction of New York, while other smaller groups with different colour-schemes were sailing in the slightly different directions of, say, Quebec or Baltimore and with somewhat smaller average velocities. But as we have said the group movements of the stars are not entirely dependent on "colour-scheme" (or spectral type), and may in certain cases depend on some such other factor as the common membership of a group of stars moving in a stream within the larger system. Clearly, therefore, the sun's motion will differ slightly according as we determine it relative to all these streams lumped together (to the swarm as a whole) or relative to the various streams separately. The first of these problems which is probably the more important of the two, has already, as we have seen, been tackled with considerable success. The second problem is still in its infancy; and, although in their pursuit of it, astronomers appear to be traversing a maze of ever increasing complexity, they will have to push ahead undiscouraged if ever they are to learn why there are these curious systematic motions among stars that just happen to share some such quality as similar spectral type.

Another phenomenon that has emerged during the study of the sun's motion is what at first sight appears to be a general tendency of the stars under consideration to be receding from us—a tendency for the nearer part of our galactic sub-system to be expanding. This is revealed by the radial velocities of the stars, which, after the solar motion has been fully allowed for, show a residual velocity of *recession* from us amounting to what we may provisionally take as approximately one mile a second.

If the sun were at rest in relation to the swarm of comparatively near stars, then in each direction in the sky taken separately the radial velocities towards and away from us would exactly cancel out. There would in any given direction in the sky be equal numbers of stars moving towards us and away from us, and the average of the velocities (plus and minus) would be zero.

If the sun were moving, as in fact we have found it to be, at about fifteen miles a second in relation to the swarm as a whole, the radial velocities would no longer cancel out in *every* direction in the sky. In the direction *towards* which we were moving the average of the radial velocities would be fifteen miles a second *towards* us. While in the direction *away* from which we were moving the average of the radial velocities would be fifteen miles a second *away* from us. Only in those directions in the sky at right angles to these two would the radial velocities exactly cancel out and the average be zero.

In point of fact this turns out to be only approximately true. It is found that the average radial velocities in the two directions towards which and away from which we are moving are not exactly equal and opposite. In the direction *ahead* the average radial velocity is about fourteen miles a second towards us, and in the direction *behind* it is about sixteen miles a second away from us. Moreover, in the directions in the sky at right angles to these two the average radial velocity instead of being zero is about one mile a second *away* from us. It will be clear that one way of explaining this result would be to suppose that in addition to the sun's velocity of fifteen miles a second relative to the swarm as a whole, the swarm itself was expanding at a rate which would give the stars an average velocity of about one mile a second outwards from the centre of the swarm.

This residual recessional velocity, which is left over after full allowance has been made for the solar motion, is known as the " K-term " in the radial velocities. Attention was first drawn to it in 1910 by Kapteyn and Frost, the then director of the Yerkes Observatory, in their investigation of the radial velocities of the B-type stars; and they put its value at nearly three miles a second. In the following year the phenomenon was fully confirmed by Professor Campbell, who gave it its non-committal name. He also determined its value both from the radial velocities of the stars as a whole and from the radial velocities of the stars of the various spectral types separately. He found this value to depend on the spectral type of the stars in the same sort of way as did the velocity of the sun itself. For the whitest and reddest stars the K-term was largest, approaching nearly three miles a second, while for the intermediate yellowish stars it dropped almost to zero.

Even this result would not necessarily weaken the explanation given above which supposes that our swarm of stars is expanding. For since as we have seen the stars of the various spectral classes are known to have their respective group velocities, there is no *a priori* reason why the tendency of the swarm to expand should not affect the various spectral classes differently—why the system of the red stars and the system of the white stars should not be expanding more rapidly than the systems of stars of intermediate colours.

But why should the swarm comprising the stars in the nearer parts of our galactic sub-system be expanding at all? Until very recently no good reason could be suggested, and the question arose as to whether there was not some other interpretation to be put upon the K-term. It can be stated straight away that the K-term cannot be regarded as a manifestation of the general expansion of the universe accounted for by relativity. The relativistic recession of celestial bodies only comes into action at distances comparable with those of the spiral nebulae; within the galaxy it plays no part, the repulsive force being neutralized by the attractive force of gravitation. Now the K-term in the radial velocities, like the radial velocities themselves, is indicated by a slight shifting of the spectral lines: in this case, since it is directed away from us, a shifting to the red. Now can this shift to the red be due to anything other than a bodily movement of the stars away from us? Three possible alternatives have been suggested.

First the red shift may be due to the downward movement of the cooler gases in the stellar atmospheres. Cool gases moving inwards from the surface of a star will clearly be moving *away* from us and so should produce a redward shift in the absorption lines. The suggestion is supported by Mr. Evershed's work which has revealed a red-shift on the sun due to this cause and amounting to about half a mile a second; and there is reason to suppose that particularly in the white stars such downward atmospheric currents might be much more rapid.

Secondly, the red-shift may be due to the Einstein effect whereby the vibrations of electrons are slowed down and the spectral lines shifted towards the red when the light emanates in the vicinity of a very intense gravitational field. We saw that in the case of the extremely dense, white-dwarf companion of Sirius the gravitational field at the surface of the star was sufficient to produce a very marked red-shift of this kind. In the companion of Sirius the amount of the Einstein shift is equivalent to a radial velocity of more than twelve miles a second. In the majority of normal stars the shift would be practically negligible, but it should become appreciable in the whiter stars and amount to about one mile a second in the B-type stars.

Thirdly, the red-shift may be partly illusory and due to small systematic errors in the laboratory determinations of the normal positions of the spectral lines; for of course the red-shift is measured by comparing the positions of the lines in the spectrum of the star with those of the same lines in laboratory spectra. For the majority of the more important spectral lines the positions as measured in the laboratory are extremely accurate; but many of the lines in the early type stars are not readily produced in the laboratory so that their positions are known with less precision.

The value of the K-term and its dependence on spectral type and other possible factors have been the subject of much investigation during recent years. In the main Professor Campbell's original results have been confirmed. All are agreed that its value is greatest for the O and B type stars and at first diminishes rapidly with advancing spectral type. It also appears to increase again for the reddest stars, but the consensus of opinion is that the increase is less than Professor Campbell and the early workers supposed, and that it never regains the high value it possesses in the B stars. Roughly speaking we may say that the K-term amounts to three and a half or four miles a second in the B and O-type stars and is of the order of one mile a second or less in the other spectral types.

It is interesting that the O and B-type stars in which the K-term is so large are the very stars in which we saw that systematic laboratory errors, the Einstein effect and downward atmospheric currents might be expected to produce the largest red-shifts. This would therefore suggest strongly that part at least of the K-term is due to one or more of these three causes. In support of this Dr. S. Albrecht of the Dudley Observatory showed in 1926 that one out of the three and a half miles a second of the K-term in the B stars could be accounted for by systematic laboratory errors. Further, both Professor Ludendorf and Professor Gerasimovic have found that the K-term is greatest in those B-stars that are brightest and heaviest, which ought to be the case with the Einstein effect and probably also with the downward atmospheric currents. And so it would appear highly probable that the greater part of the K-term in the whitest stars is due to a mixture of these three causes, and that when they are allowed for we are left with a small residual velocity of about a mile a second which is roughly the same for all spectral types and is most reasonably regarded as a general recession of the stars from the sun.

It might seem that the suggestion that the stars of our swarm are receding unanimously from the sun attributes undue importance to the status of the sun and savours unpleasantly of the geocentric attitude. This, however, is not so. In a swarm of stars that is tending to expand in size, the average outward velocity of the stars which are further from the centre of the swarm will be greater than that of the stars which are nearer to the centre of the swarm. Consequently wherever the sun happens to be placed in the swarm—even though it is in quite an unprivileged position some distance from the centre—the tendency will be for the motion of the stars relative to it to be one of recession in all directions. Thus any part of the K-term which is taken as due to a real motion of the stars, need indicate nothing more than an expansion of our swarm and in no way upsets our ideas of celestial equality. But as to the significance of the expansion we have very little idea. Particular attention has been given to the matter by Dr. von der Pahlen of Potsdam and by Professor Freundlich, who recently vacated the directorship of the Einstein Institute in Berlin for that of the Observatory of the Turkish Government in Stamboul; and their view is that expansion of the kind described does not exist. Most authorities however find evidence for it, but do not agree with one another as to their explanations. Some attribute it to the chance arrangement around us of a few predominating streams of stars whose motions just happen to be directed away from us. Some believe that the galaxy as well as rotating is slowly expanding; and that the observed local expansion in our vicinity is but a part of the general expansion on the larger scale. Finally it has been put forward that the swarm of stars surrounding us lies in one of the arms of the spiral nebula which our galaxy is supposed to be, and that the observed expansion is due to the motion of the swarm outwards along the arm, the advanced portions travelling faster than the less advanced portions. For it is probable that the stars in a spiral nebula not only share in the general rotation of the nebula but also move gradually outwards along its arms. It follows that our knowledge of the K-term is still far from complete; but it is probable that while the larger part of it in the white stars is due to a mixture of the three causes we have discussed, there remains a small part due either to a local or general systematic motion of the stars.

CHAPTER XXI

RECENT WORK ON STELLAR MOTIONS

IN the preceding discussion of the individual motion of the sun it has been impossible to avoid saying a good deal about the individual motions of the stars, for, as we have seen the two are inextricably bound together. Yet the motions discussed were strictly limited to *group* motions, the motions of the red stars, or of the yellow stars, or of the white stars, each as a group relative to the sun. There still remains another kind of individual motion: the motions of the *individual stars* of such a group relative to that group as a whole. It is a motion of this sort that is exemplified in the star-streaming discovered by Kapteyn in 1904 and already referred to in Chapter X. We shall now return to this matter in order to complete our review of the numerous complex motions which take place inside our galaxy.

In investigating the solar motion—or what came to the same thing the group motion of the nearer stars relative to the sun—astronomers proceeded in the following manner. For each of a large number of small areas all over the sky they determined the direction and amount of the average proper motion of all the available stars in the area. The result represented the effect on the apparent movement of the stars in that area for which the solar motion was responsible. It was, however, by a different procedure that Kapteyn was led to his discovery of star-streaming. For each of a large number of areas in the sky he counted the number of stars that were moving towards the different points of the compass—the number that were moving in directions lying between due north and ten degrees east of due north, the number that were moving in directions lying between ten degrees east of due north and twenty east of due north, and so on through thirty-six equal sectors till he had boxed the compass. He found that in each area of the sky there were always two directions out of the thirty-six in which a preponderating number of the stars moved. But more interesting still, the two directions given by each of the various areas converged respectively on two definite points in the heavens. If one labelled the two preferential directions in each area, I and II respectively, one found that all the Is converged upon a point south-east of Orion and all the IIs on a point in the southern constellation, Pavo. The convergence of the IIs was not so well-defined as that of the Is, although it was much too close to be ascribed to pure chance. In other words, there seemed to be two points in the sky singled out from all others towards which the stars showed a preference for moving.

One interpretation of this was that the stars, forming the subject of the investigation, belonged to one or other of two clusters of stars which were in process of passing through one another. If we imagine two stars or clusters of stars moving relatively to one another in empty space, then the only thing that can be said about their motion is that they are approaching one another, receding from one another, or passing through one another *along a certain line.* The question whether an approach, for example, is " head on " or " oblique " does not arise so long as the two stars or clusters are alone considered. On the other hand an observer who is not partaking of the motion of either of the two stars or of the two clusters will find that their motions relative to him are *not* along the same line and *are* in fact inclined obliquely to one another. Thus any obliquity in the motions of the two clusters can only exist from the point of view of an observer who is moving independently of both; while the amount of the obliquity will enable the observer to determine his own motion relative to the average of the two clusters considered together.

Now this is exactly what happens in the case of Kapteyn's two star streams. The two directions in the sky towards which they are apparently moving are not exactly opposite to one another and so are *not* in the same line. But so soon as one allows for the motion of the observer—to wit, the motion of the sun and solar system as determined independently by the methods discussed above—the directions in space towards which the two streams are respectively moving do turn out to be exactly opposite to one another. And the very fact that the answer comes out correctly—for as we have seen any other answer would necessarily be wrong—strongly supports the reliability of our determination of the sun's motion.

It will now be seen why the determination of the sun's motion—its motion in relation to the average of the stars as a whole—did not reveal the existence of the two star streams. For since the two star streams together comprised the " stars as a whole," that determination merely gave the sun's motion in relation to the average of the two streams—the very motion in fact which gives rise to the apparent obliquity of the streams.

As we saw in Chapter VII the first remarkable thing that was noticed about the streams was that the fundamental line indicating their two opposite directions of motion in space lay exactly in the plane of the Milky Way. Thus stream I is moving towards a point in the Milky Way close to where it passes the left upper shoulder of Orion; while stream II is directed to the opposite point in the sky, to that intensely bright part of the Milky Way in Aquila which we see in England low in the south in summer. This itself seemed to suggest that star-streaming was more than a mere local phenomenon. For two minor clusters in process of passing through one another would hardly be likely to hit each other off in a direction so precisely in the fundamental plane of the galaxy. It looked as if star-streaming had a wider and grander significance.

It was however pointed out by Sir Arthur Eddington that it was just possible that star-streaming might in a sense be illusory. It was conceivable that the two streams were at different distances from us: that the stars of the nearer stream were members of a cluster surrounding the sun, and that this cluster was simply moving relatively to the background of the more distant stars which thus appeared to be members of a second stream. And so in order to exclude this obvious possibility Sir Arthur determined the average distances of the stars in the two streams. As he expected the distances turned out practically the same, showing conclusively that the two sets of stars were really intermingled in space and that streaming was a real phenomenon.

There was still the question as to how far streaming might be a local phenomenon confined to our immediate neighbourhood; and although as we have seen this appeared to be improbable, it was desirable to obtain direct evidence on the matter. The earliest investigations in this connexion were those of Sir Arthur Eddington and Sir Frank Dyson between 1906 and 1908. They extended the studies of Kapteyn to the proper motions of much fainter and thus on the average of more distant stars, and showed that star-streaming was still in evidence.

So far star-streaming had been studied from proper motions alone. Yet if the stars have a preferential motion towards two points in the sky, then after allowing for the sun's motion the average radial velocities either towards or away from us ought to be greatest in these two directions in the sky. As early as 1909 Hough and Dr. Halm at the Cape attempted to detect such an effect, and although they succeeded their data were scarcely extensive enough to make their result convincing. Shortly afterwards Professor Campbell got rather stronger evidence of the expected effect. But it was not until 1915, when the problem was tackled by Professor Campbell again and independently by Professor Adams in collaboration with Kapteyn, that the twofold streaming of the stars was shown up clearly from radial velocities. The last two investigators also found that there was no diminution of the streaming tendency with increasing distance; and since the study from radial velocities in any case embraced more distant stars than did the study from proper motions, the case for star-streaming being a widespread phenomenon gained considerably in strength.

Meanwhile it was becoming apparent that star-streaming was in some way dependent on spectral type. But before taking up this point we must consider that important relation between the spectral types of the stars and their haphazard velocities in space, which was briefly referred to in Chapter X in connexion with the degree of equipartition of energy so far achieved in the universe. It will be remembered that the suggestion was made that the energy of motion of every star was approximately the same—as in the case of the molecules of a gas—so that the heavier a star is, the more slowly it moves; and the origin of this suggestion was the curious fact that as one passed down the main spectral sequence, from the white stars to the red dwarfs, which was also a sequence of diminishing mass, the space velocities of the stars (irrespective of direction) increased progressively. The first intimation of this speeding up with increasing redness had been given in 1892 by Monck who noticed that in general the proper motions of the red stars were larger than those of the white stars. This conclusion was subsequently re-emphasized by Kapteyn and Sir Frank Dyson; but since the proper motions are only apparent motions, it did not necessarily follow that the red stars were really moving faster than the white ones: it might have been that on the average they were nearer to us. In 1903 Frost and Professor Adams from their measurements of radial velocities at the Yerkes Observatory drew attention to the extraordinarily small velocities of the white B-type stars. That this was a particular case of a more general law was not, however, realized until 1910. In that year Kapteyn and Professor Campbell showed independently from radial velocity measurements that the average random velocities in space increased progressively from about four miles a second for the white B stars to about eleven miles a second for the red stars; and about the same time practically identical figures were obtained by Lewis Boss from an analysis of the proper motions. Since the recognition of giant and dwarf stars, the above statement of the rule has had to be slightly modified. A definite increase in speed with increasing redness applies only to the *main* spectral sequence, as one passes from the white stars to the red dwarfs, and does not include the giants. As one would expect the much heavier giants move more slowly than do the dwarfs of the corresponding spectral type.

The velocities discussed in the last paragraph refer to the *random* motions of the stars, their movements through space in *any* direction whatsoever. They thus have nothing to do with star-streaming and are equally to be distinguished from such group motions as were considered in relation to the solar motion. These distinctions will be made clearer by an example Let us contrast the movements of two kinds of crowds: a crowd during a garden-party, and a crowd immediately after a football match. In both there will be the random movements of the individuals, their movements with diverse speeds and in diverse directions relative to the centre of gravity of the crowd. We may liken the white stars to the garden-party crowd in which the individuals walk leisurely to and fro; and the red stars to the football crowd in which they run about and bustle. In both there will be the *group* motion, the movement of the crowd as a whole. At the garden-party the group movement may be zero, the centre of gravity of the crowd remaining stationary on the lawn. But in the football crowd, immediately after the game, the group motion may be rapid; for the centre of gravity of the crowd will be moving along from the football ground to the station. Finally in both there may or may not be a streaming effect. For argument's sake we assume that the people at the garden-party are entirely lacking in any purpose other than that of passing the time, and so will wander in and out among the crowd in all directions indiscriminately: there will be no line (or lines) of preferential motion, the velocities in all directions will cancel one another out. But in the football crowd the individual motions relative to the centre of gravity of the crowd will not be entirely at random. The people will be moving to and fro in all directions in relation to the centre of the crowd; but since everyone is hurrying to get the best seats in the train, the most vigorous will push ahead of the centre of the crowd, while the weaker members will lag behind it. There will in fact be a preferential motion relative to the centre of the crowd in two opposite directions, in the direction of the station and in the direction of the football ground; and this preferential motion will lead to a progressive elongation of the crowd along this particular line. In the example taken, the random motion, the group motion and the streaming effect are all large in the one case and all small in the other; but it is quite obvious that the three need not necessarily run parallel. It would be easy to find examples in which any one of them was large and the other two were small, or in which any one of them was small and the other two were large.

We saw just now that the random motions were smallest among the B stars and got progressively larger for stars of increasing redness. On the other hand it was found earlier that the solar motion was most rapid in relation to the B stars, less rapid in relation to the yellowish stars and more rapid again in relation to the red M stars. That is, relative to the sun the group motion was greater for the B and M stars than for those of intermediate type. But the velocity of a group motion obviously depends on relative to *what* it is judged; and it would seem more rational to judge the group motions of the various spectral types in relation to the average of the stars as a whole, rather than in relation to the sun. And when this is done, by first of all allowing for the solar motion relative to the average of the stars as a whole, it is found that the group motion of the B stars relative to the sun is entirely accounted for by the solar motion—that in fact the B stars relative to the stars as a whole are practically stationary as a group. It follows that from the standpoint of the swarm comprising the nearer stars of our galactic sub-system, the redder stars which have the highest random velocities have a comparatively small group motion, the yellowish stars which have moderate random velocities have the greatest group motion, and the B stars which have the smallest random velocities have practically no group motion. So much for the group and random motions, let us now see in what way the third type of motion, the preferential motion or streaming effect, may likewise be dependent upon spectral type.

The first investigation bearing upon this matter was Professor Campbell's analysis in 1911 of the radial velocities of the B stars. He was able to conclude that they showed no evidence at all of preferential motion, their small random velocities being equally distributed in all directions in space. That very same year Dr. Halm at the Cape made the interesting discovery that if the proper motions of the stars were studied more critically there appeared definite evidence for a third stream, which in most parts of the sky tended to be obscured by Kapteyn's stream I. When the solar motion was not allowed for, the directions of the three streams met each other obliquely. But when the stream motions were freed of the solar motion, so that streams I and II became directed as we saw in opposite directions along a straight line, the new stream was seen to be stationary. In other words it seems that the stars of the swarm under consideration belong to one or other of three streams: stream I is moving in a certain direction at about nineteen miles a second relative to the centre of the swarm; stream II is moving in the opposite direction at about ten miles a second relative to the centre of the swarm; and, finally, the new third stream is at rest relative to the centre of the swarm. But what was specially interesting, particularly in connexion with Professor Campbell's radial velocity results, was Dr. Halm's discovery that, though his new stream contained stars of every spectral type, it contained *all* the stars of type B. So by two quite independent methods the B stars were shown to be exempt from star-streaming.

The third stream falls into a somewhat different category from the other two. Streams I and II each contain stars of the commoner spectral types in roughly the same proportions, and so are in no way distinguished by the physical characteristics of their membership: they are to be distinguished solely by their motions. But the new stream in addition to containing a share of the commoner spectral types contains all the stars of the one type B: it is thus characterized not only by its motion—or state of rest—but also by a physical peculiarity of its membership.

Following the discovery of the absence of the preferential motion in B stars investigations were extended to the other spectral classes. Lewis Boss tackled the problem via the proper motions, while Professor Adams, Kapteyn, Dr. Gyllenberg in Sweden, Sir Arthur Eddington and others approached it through the radial velocities. The results show that as one passes from the B to the A stars, which come next to each other in the spectral sequence, there is a sudden jump in the preferential motion from nil to its maximum value; thereafter the preferential motion seems to fall off, but only slightly, as one passes on to the redder stars. In 1925 a still more detailed study of the matter was made by Dr. Strömberg at Mount Wilson; and while his results were in agreement with those just quoted, he found that the degree of the preferential motion was more definitely dependent on the absolute brightness of the stars than upon their spectral types. For any given spectral type, the preferential motion was more exaggerated the absolutely fainter the star was, while it was least marked among the absolutely brightest stars of each spectral type. We saw from the mass-luminosity law that the weight of a star and its absolute brightness went closely hand in hand, and we have also seen that as the weights of the stars diminish their random velocities in space increase. It thus follows from Dr. Strömberg's analysis that the preferential motion becomes increasingly manifest not only as the absolute brightness diminishes but also as the velocities of the stars increase. The complete absence of preferential motion among the B stars thus fits in both with their exceptionally great absolute brightness and with their exceptionally small random velocities.

In all the investigations on the solar motion and on the individual motions of the stars so far considered the data from the " high velocity stars " had by agreement been excluded. We saw that this arrangement had seemed reasonable on the grounds that such investigations, being essentially concerned with the *general run* of the stars, ought not to be influenced by stars whose exceptionally high velocities showed them to be definitely abnormal. No official decision had ever been made as to where precisely the line dividing the high from the low velocity stars should be drawn; but it had become the custom to exclude from the data all stars with velocities over thirty or forty miles a second, which is three or four times as great as the average random velocity of the red M stars.

In this investigation of Dr. Strömberg, however, the velocity limitation had been removed. In it he had considered the group motion relative to the sun and the preferential motion of every class of star from the most slowly to the most rapidly moving. He took as we saw the various spectral types separately and subdivided each of them into several groups, ranging from the most slowly moving and absolutely brightest stars to the most rapidly moving and absolutely faintest stars; and he also took certain additional groups, which fell naturally into special classes, such as the shorter period Cepheids, the longer and shorter period long-period variables, and the globular clusters—the various " high velocity bodies."

This investigation by Dr. Strömberg is the one already referred to in connexion with galactic rotation, which led to his discovery of the asymmetry in the motions of the high velocity bodies. It will be recalled that the various groups of high velocity bodies were found to have group motions relative to the sun quite different from those of the ordinary stars. Although the speeds with which these high velocity groups were moving relatively to the sun differed largely from one another, the direction in which they were moving relatively to the sun was the same for all—and yet this common direction was entirely different from that in which the low velocity stars were moving relatively to the sun. In other words, the high and low velocity stars gave quite definite but quite different directions for the solar motion. This was taken to mean that the high velocity groups represented sluggishly revolving galactic sub-systems which were lagging behind our own; and this was supported by the fact that the common direction in space towards which the group motions were directed—the direction towards which they were lagging—was precisely opposite to that towards which the sun was travelling in its revolution round the galactic centre. On the other hand the low velocity stars are regarded as members of the same galactic sub-system as the sun belongs to; hence the motion of the sun relative to these stars will represent its motion relative to the nearer parts of its own galactic sub-system, namely what we have previously referred to as "the sun's individual motion." The direction of the sun's motion given by the ordinary low velocity stars—the direction in which it is travelling through its own sub-system—is represented by a point in the sky somewhat south of the bright star Vega. On the other hand the direction of the sun's motion given by the high velocity stars—the direction towards which it is travelling in its galactic orbit—is about thirty degrees further north towards a point in the constellation Cepheus.

But the chief interest for us at the moment is the bearing of this investigation on the preferential motion of the stars. Dr. Strömberg found, as we saw, that the preferential motion was more marked among the faster moving than among the slower moving stars in the various spectral types; but the point is, did it continue to become more marked after one had passed the forty mile limit and reached the high velocity stars? His answer was that it did. In a few of the special groups, like the shorter-period Cepheids, the long-period variables and the globular clusters, the preferential motion was slight though definite. But in the groups consisting of the fastest moving stars of the various spectral types—groups in which the random velocities well exceeded the forty mile limit and in which the group motion showed that characteristic asymmetry which gave a solar motion in the direction of Cepheus—the preferential motion was very striking.

This result was of capital importance: it meant that although the high and low velocity stars were travelling in different direction relatively to the sun, the directions of their preferential motions were parallel to one another. Let us imagine two groups of people moving across a field in two different directions: a high velocity group, in which the individual velocities are large, moving rapidly (i.e., with a large group velocity) due north; and a low velocity group, in which the individual velocities are small, moving more slowly (i.e., with a small group velocity) north-east. Imagine further that in both groups the individual motions are not entirely at random but in each tend to be more numerous in two opposite directions along a certain line, so that the groups tend to become drawn out and elongated in this direction. Finally, let us suppose that in the case of the high velocity group the line of preferential motion is at right angles to the group motion and is thus directed due east and west; then, to complete the analogy, the preferential motion in the low velocity group will also have to be directed due east and west in spite of the fact that the group motion in this case is north-east.

This persistence of the preferential motion even among the high velocity stars and also the fact that its direction is independent of the direction of the group motion, affords additional evidence that star-streaming is a widespread if not a universal phenomenon within the stellar system. At least it would seem to imply that star-streaming extends beyond the limits of our own galactic sub-system. Another matter which points to star-streaming being a fundamental property of the stellar system is the fact that its direction is approximately at right angles to the direction of the group motion of the high velocity bodies. And as to the fundamental significance of this last direction there cannot be the least shadow of doubt; for, apart from the correctness or otherwise of our interpretation that it is the direction opposite to that in which the sun is revolving in its galactic orbit, it stands out, simply from the observations, as the common group direction of all the most remote and widely distributed classes of celestial bodies in our galaxy.

What fresh light, if any, do these various recent studies throw on the phenomenon of star-streaming? The two physical interpretations put forward up to about 1914, which were discussed in Chapter VII, now appear less plausible. The passage through one another of two open galactic clusters, in the midst of which we happen to be, seems to localize the phenomenon much more than the observations warrant. The suggestion that the preferential motion represents a two-way current running between the points where the two spiral arms enter the central nucleus of a spiral nebula, requires that we are situated much nearer to the centre of the galaxy than we now know ourselves to be.

In 1908, four years after Kapteyn's discovery of the preferential motion and his explanation of it by means of the two star streams, a somewhat different theory was put forward by Schwarzschild of Gottingen. According to this there was no occasion to postulate two physically independent streams of stars passing through one another: it was sufficient to assume that, in a single system of stars, the motions were more frequently parallel to one particular line than to any other line. On the face of it this so-called *ellipsoidal hypothesis* seems to express in somewhat different terms much the same idea as is contained in the two star stream theory. In their physical interpretation the two theories are at least superficially similar; but the ellipsoidal hypothesis has the advantage over the other of being much more convenient for the mathematical treatment of the observations. Hence although Schwarzschild's theory soon superseded Kapteyn's for all analytical purposes, the latter with its underlying idea of a two-way stream continued to form the basis of the various explanations put forward in interpretation of the phenomenon.

But the difference between the two theories is not so subtle that it has no physical significance at all: quite a simple analogy should make it clear. Let us suppose that a boys' school and a girls' school each out for a walk have inadvertently run into one another, say, on the Downs outside Eastbourne. For a time the two schools are completely intermingled and form a more or less circular swarm. In it the individual motions which are largely chaotic will have a tendency to be most numerous in two opposite directions along a certain line—the line of meeting and subsequent separation—so that the swarm gradually becomes elongated until it again divides into two. It is during the stage of the complete mix-up that the two schools best represent the two star-streams as pictured by Kapteyn; for, owing to the efforts of the teachers in charge, the boys and the girls exhibit preferential motions in two directions respectively opposite to one another.

Let us now consider a single school that is out for a Sunday walk on the Eastbourne Downs. We shall suppose that all the children start together in a crowd, and although they are made to follow closely a fixed route they are permitted considerable latitude as regards the time of their return. Consequently the swarm, which was originally more or less compact and circular, will gradually become drawn out as the energetic children run on ahead and the lazy ones lag behind. But in addition to running on ahead or lagging behind the children will be running off to the sides to pick flowers, etc. Thus the individual motions relative to the centre of the swarm are taking place in *all* directions; and it is only because of the special circumstances we have assumed—the *narrow* limits permitted in the route, and the *wide* limits permitted in the time taken over it—that there is a preferential motion, and a consequent elongation, of the swarm in a direction parallel to the route which the children are taking. This picture represents the underlying idea in the ellipsoidal theory: the appearance in a single swarm of two streams moving in opposite directions relatively to its centre.

In the above analogy the circumstances assumed produce a preferential motion that is parallel to the group motion; but this is not essential. We might have supposed that the children were instructed not only to start together but also to return together. To insure this, one teacher might walk in front and another behind; so that while the children could no longer straggle behind or go on ahead they would still be free to make their lateral flower-picking excursions. We should then have a preferential motion and an elongation of the swarm in a direction at right angles to the group motion—just as we found to be the case among the high velocity stars.

It will be seen that the fundamental difference between the two theories is this: in Kapteyn's theory the preferential motion is the result of an essential duality in the swarm of stars under consideration; whereas in Schwarzschild's theory the preferential motion emerges despite the absence of any such duality. It follows that on Kapteyn's theory the two streams *might* be expected to show some physical distinction between their respective members—just as the members of one of the intermingling schools might differ in age, dress, or sex from the members of the other; whereas on Schwarzschild's theory any physical distinction would be less likely to be present. Thus the fact that the members of the two star streams are, as we saw, physically indistinguishable—by spectral type, etc.—is itself a point somewhat in favour of the ellipsoidal hypothesis.

We have seen that the recent work on the preferential motion has tended to support the view that it is a widespread phenomenon possibly affecting the galaxy as a whole. If that is so it is difficult to see how we are to explain it by means of an hypothesis that requires an essential duality in the system. It might of course be supposed that our galaxy is a double galaxy consisting of two galaxies that happen to be passing through one another. But the explanation is most improbable for we know that the distances between the individual spiral nebulae are very great and that the chances of two of them colliding are very small. Thus if we are right in supposing the preferential motion to be a general galactic phenomenon, a more reasonable explanation would appear to be one that is based on the general pattern of the ellipsoidal hypothesis.

A theory of this type was first put forward in 1912 by H. H. Turner, the then Savilian Professor of Astronomy at Oxford. He pointed out that if we analysed the motions of the comets, which mostly revolve round the sun in highly elongated orbits, we should find that their preferential motion was directed radially towards and away from the sun. He suggested therefore that the stars in our galaxy might be revolving round the galactic centre in highly elongated orbits, in which case observation should ultimately show that the direction of the preferential motion was in the directions towards and away from the galactic centre.

Shortly afterwards Kapteyn and others suggested an alternative solution: they supposed the stars to be moving in almost circular orbits round the galactic centre, that the preferential motion was the result of the going on ahead of the quickly moving stars and of the lagging behind of the slowly moving stars, that in fact observation would ultimately show that the preferential motion was along a line directed at right angles to the direction of the galactic centre. It was six years later, in 1918, that observation did ultimately decide between these two diametrically opposed solutions. In that year Professor Shapley's investigation of the distances and distribution of the globular clusters had enabled him to define the two opposite directions in the sky towards and away from the galactic centre: they coincided closely with the directions of the two star streams. But although observation had decided definitely in favour of Turner rather than of his opponents the matter could not yet be regarded as settled. So far there was absolutely no direct evidence for the rotation of the galaxy; and the most one could say was that if star-streaming *was* a manifestation of galactic rotation, then it was probably the result of the elongation of the stellar orbits and not the result of the differing velocities of the stars in circular orbits.

In the last ten years the rotation of the galaxy has been proved beyond all doubt and much has been learned as to the mechanism of that rotation. It has thus become possible to examine Turner's theory more critically and to see whether in fact it is compatible with what we know about galactic rotation. We saw that according to the views of Professor Lindblad and Dr. Oort, the vast majority of the stars belong to the same galactic sub-system as we do, and revolve in orbits that are approximately circular: it is only a minority of the stars, those which belong to other sub-systems, that *may* move in highly elongated orbits. Yet it was an essential point in the explanation of star-streaming, as originally put forward by Turner, that the stars should move in highly elongated orbits. So it might have appeared that after all Turner's theory was not to be squared with the facts. But Professor Lindblad and others have shown that even if the stellar orbits are only slightly elongated the slight departure from circularity will suffice to produce a preferential motion towards and away from the galactic centre.

Presumably the stars are revolving in their orbits at speeds which counteract their tendencies to fall inwards under the general gravitational attraction of the galaxy—exactly as is the case with the planets and comets of the solar system. Stars that are travelling in perfectly circular orbits and are at approximately the same distance from the galactic centre, will all have the same velocity. We have seen that at the distance of the sun from the galactic centre this velocity is about 170 miles a second. Thus if all the nearer stars of our own particular sub-system were travelling in circular orbits, they would all be moving parallel to one another at 170 miles a second, and there would be no individual motions (and therefore no preferential motion) among them to detect. If however their orbits are apt to depart even very slightly from perfect circles then immediately individual, or relative, motions will appear. The velocities of the stars moving in slightly elongated orbits will sometimes be greater and sometimes less than that of the stars moving in circular orbits: so one effect of the existence of slightly elongated orbits will be that some stars will lose, and others gain, on those that are moving with the ordinary circular velocity. But there is also another effect: a slightly elongated orbit will necessarily deviate slightly from the course of the perfect circle—either inwards towards, or outwards away from the galactic centre. Hence the individual motions that arise will occur not only in the two directions forwards and backwards along the orbit in which the sub-system is revolving, but also in the two perpendicular directions towards and away from the galactic centre. Moreover it can be shown that this second effect will be bigger than the first, that the existence of slightly elongated stellar orbits will cause the individual motions of the stars to be most marked in the directions towards and away from the galactic centre.

And so at last after about a quarter of a century a satisfactory explanation of star-streaming has been provided; and the growing suspicions of recent years that it was the result of some fundamental property of the galaxy, for example rotation, have been justified. It is interesting also to note that the new theory accounts for the curious behaviour of the B stars, the chief members of the third " stationary " star stream. As long ago pointed out, the B stars exhibit a galactic concentration more marked than that of the stars of any other spectral type. But according to Professor Lindblad, members of sub-systems having the greatest galactic concentration should move in the most nearly circular orbits: they should thus show as we have just seen the smallest individual motions and the least marked preferential motion—precisely, in fact, the characteristics of the B stars. Stars of other spectral types showing less galactic concentration will tend to move in less perfectly circular orbits, and thus will show preferential motion inwards and outwards relatively to the circularly moving stars. Consequently such stars will belong to one or other of two main streams directed respectively towards and away from the galactic centre; while intermediate between them, and at rest relatively to the average of both of them, will be all the circularly moving stars which include all the B stars. We thus have an explanation of our third stationary star stream, whose membership comprises all the B stars and a proportion of the stars of other spectral types.

We must unfortunately terminate this discussion of stellar motions on a somewhat less secure note. There has existed for some years a slight discrepancy between the observed directions of star-streaming and the observed directions of the galactic centre and anti-centre. At first the discrepancy was ascribed to imperfections in the observational material; but now there is no doubt that it is real and that it cannot be brushed so lightly aside. The most likely explanation of it, which we owe mainly to Professor Lindblad and to Dr. Mineur of Paris, is that the nearer portion of our galactic sub-system, in addition to sharing in the general galactic rotation, is itself rotating about its own centre of gravity—just as the earth and moon while revolving together round the sun also revolve about their common centre of gravity. It is probable that this nearer portion of our galactic sub-system is to be identified with the " local cluster " of Professor Shapley and Charlier. Charlier's studies of the distribution of the brighter B stars had led him to the conclusion that our local cluster was a bun-shaped system of stars, with the sun some little distance from its centre, having a diameter of about 2,000 light-years and a thickness of about 700 light-years. The local cluster is believed to be just one of the many galactic clusters; and though it is considerably larger than the ordinary open clusters, it is probably comparable in size with the great star clouds of the Milky Way.

Although Dr. Mineur has produced a good deal of evidence in favour of the rotation of this cluster about its centre of gravity the matter is still far from being definitely decided. The best evidence for it is that it *would* account for that slight deviation between the direction of the galactic centre and the direction of star-streaming. There is, however, one piece of indirect evidence that should carry considerable weight. We saw that since the beginning of things the galaxy must have made several hundred if not several thousand complete rotations. Now since the rate of rotation is more rapid the nearer one is to the galactic centre, it follows that in a cluster that is at all extensive the parts of it that are nearer to the galactic centre will revolve more rapidly than the parts of it that are further from the galactic centre. In other words the rotation of the galaxy ought to have a sort of " shearing effect " on all star-clusters, which after a very few revolutions would pull them out into elongated swarms—the parts of the cluster further from the galactic centre lagging behind the nearer parts. Now it is quite obvious from the appearance of the star-clusters that this has not happened. But the only way in which the shearing of star-clusters could be avoided—apart from the possibility that our ideas on galactic rotation are all wrong—is that the clusters themselves are sufficiently compact for their respective gravitational fields to hold their individual parts together. And if that were so each cluster would of necessity be rotating about its own centre of gravity.

Perhaps the most remarkable thing about these studies of the structure and mechanism of our galaxy is that they have taken us so far. There is much of which we are uncertain and still more of which we are completely ignorant. Yet when we realize the smallness of the globe to which astronomers are confined and the immensity of the system which they have set out to explore, we cannot but wonder at the success they have so far achieved. And since it is particularly during the last twenty years that the knowledge of our galaxy has made such strides, we may be thoroughly optimistic about the future.

CHAPTER XXII

THE SPIRAL NEBULAE

SINCE the island-universe theory began to be established about seven years ago the weak point in it has always been that our own galaxy came out so very much larger than any of the others. The situation was somewhat relieved by the investigations on galactic absorption; for when due allowance was made for the absorbing medium the previous estimates of the diameter of the galaxy had to be reduced by more than one-half, while the distances and dimensions of the spiral nebulae remained unchanged. Even so, the final diameter of the galaxy, some 90,000 light-years, was still about twice as great as that of the Andromeda nebula—a system which, as we shall see, is itself an exceptionally large spiral.

It is desirable therefore that we should now carry the comparison of our own galaxy with the other galaxies to its most recent stage. We shall start by comparing the galaxy with its nearest neighbour, the Andromeda nebula, and afterwards compare it with the spirals in general. Moreover, we shall consider the likenesses and differences in structure as well as in dimensions. In this way we shall have an opportunity to say something about the various types of spiral nebulae and their possible significance in regard to stellar evolution.

It must be remembered that the estimates of the dimensions of our galaxy so far considered were strictly speaking estimates of the dimensions of the enveloping system of globular clusters. There is no doubt that the flattened concentrated disc of stars that forms the Milky Way does represent the equatorial plane, so to speak, of the globular cluster system; but on the other hand there was no definite evidence that the flattened disc reached as far as the periphery of the enveloping system. In our model of the buttered bun it may be that the butter has not been too generously spread, and is lacking round the edge of the ought-to-be-buttered layer. We saw, however, that the sun is about three-fifths of the way from the centre to the edge of the system of globular clusters, and we know that the flattened disc of stars extends well outwards beyond the sun; consequently the diameter of the disc cannot fall very much short of that of the enveloping system. Nevertheless we must keep in mind the possibility that the stellar system proper—the stars, star-clouds, open clusters and nebulae of the

Milky Way—may have a diameter appreciably smaller than that of the system of globular clusters.

In 1932 Dr. Hubble made a discovery bearing directly on this subject as well as on that of the similarity of the galaxy to the Andromeda nebula. On long exposure photographs of the Andromeda nebula taken with the 100-inch telescope he found surrounding it about 140 extremely remote globular clusters. Thus the Andromeda nebula was provided with a system of globular clusters of its own, analogous to that of our own galaxy. Moreover, he found that the clusters extended well clear of the ordinary photographic limits of the great spiral, and in fact about doubled the diameter of the system as previously recognized.

In 1934 another conspicuous advance was made, this time by the Harvard observers under the leadership of Professor Shapley. Two years earlier, because of the increasing smokiness of the Cambridge skies, a new photographic outpost of the observatory had been set up some miles out in the country at Oak Ridge. Here, with the aid of rapid cameras and long exposures, the Harvard observers were able to obtain photographic images of the spiral nebulae which were much more extensive than any that had been obtained hitherto. And furthermore, when these very fine photographic images were examined with an automatically recording photometer—instead of, as ordinarily, with the eye—their diameters were found to be two or three times as large as had previously been supposed. In the case of the Andromeda nebula, for example, its faint outlying parts were found to extend as far as or even farther than the outermost of the globular clusters discovered by Dr. Hubble.

Thus the diameter of the system of the Andromeda nebula was increased to at least 60,000 light-years and probably more. And we can say quite definitely that as far as this particular spiral nebula is concerned its dimensions cannot be significantly different from those of our own galaxy.

This work at Harvard re-emphasizes a matter already mentioned, that a galaxy possesses no such thing as a definite border. At increasing distance from its centre the stars gradually peter out. Strictly speaking the only way in which one can define its " edge " is by saying it is the region where the crowding together of the stars has fallen below, say, one-tenth, one-hundredth, or one-thousandth of their crowding together at the centre. Unfortunately this is impracticable. Instead one has to make use of standard exposures taken with the same instrument and measure the diameters of the images by a standardized method. And though in this way one will get results that are in general satisfactory for the comparison of the diameters of the spirals *inter se*, it does not help one to escape the difficulty as it affects the comparison of the diameters of the spirals with that of our own galaxy. The best that can be done is to appreciate the existence of this difficulty and try to make allowance for it. It may be, for example, that though our galaxy has a diameter of about 90,000 light-years the crowding together of the stars in the outer rim of it is considerably less than that of the stars in the outermost detectable rim of the Andromeda nebula. In other words, we may tend to over-estimate the dimensions of our own galaxy and underestimate those of the spiral nebulae.

An investigation relating to this matter has been in progress for some years at Harvard and its station near Bloemfontein. A search is being made for very remote Cepheid variables, which are unassociated with globular clusters, in order to get some idea of the extent of the stellar system proper as opposed to the system of globular clusters. The first part of the investigation is concerned with the extent of the stellar system in the plane of the Milky Way; the second part is concerned with its thickness, its extent above and below that plane. Since the distance of a Cepheid is determined from the period of its light variation and from its average apparent brightness, it is essential that any effect on the latter from galactic absorption should be correctly allowed for. But if one is out to discover the extent of the stellar system in the direction of the plane of the galaxy it is obviously necessary to look for Cepheids as near to this plane as possible—in the very directions in fact where the galactic absorption is strongest, most irregular and least accurately determined. Consequently one must go as close as possible to the Milky Way without going too close. Now from the work of Dr. Hubble and others there is no doubt that whereas the galactic absorbing layer is very shallow in our own neighbourhood and in most directions, it is swollen out to a much greater thickness in the vicinity of the galactic centre. Thus while one can approach quite close to the Milky Way in the greater part of its extent in comparative safety, one is bound to encounter serious danger, even at considerable distances from it, where it lies in the direction of the galactic centre. So very wisely Professor Shapley refrained from investigating the extent of the stellar system, as given by individual Cepheids, in the direction of the galactic hub, and confined himself to investigating its extent in the opposite direction away from the hub. It will be remembered that the most recent work on the distribution of the globular clusters placed the solar system about 15,000 light-years from the periphery of that system. The new investigation from Harvard compares very interestingly with this result. Of the fifty-two Cepheids fairly close to the Milky Way in the direction of the galactic anti-centre three-quarters were within 15,000 light-years of the sun while the remainder were very sparsely scattered up to a distance of nearly 40,000 light-years. We cannot of course argue from this to the stars in general; in fact it is possible that the Cepheids are much more widely scattered than the average run of stars. But it shows that in the direction away from the galactic centre the extent of the system as given by the Cepheids is approximately the same as that given by the globular clusters, while an occasional straggler may be found out to much greater distances. It illustrates also the indefiniteness of the term "edge" as applied to galaxies.

The second investigation, relating to the thickness of the galaxy, brought out even more clearly this indefiniteness of the confines of the stellar system. In the directions at right angles to the galactic plane the methods based on star counts give a fairly definite idea of the rate at which, above and below this plane, the general run of stars as well as stars of various spectral types fall off in numbers. At distances in excess of about 500 light-years above and below the galactic plane the compactness of the stars begins to diminish rapidly, until at distances of about 1,500 light-years the stars are extremely few and far between. As regards the general run of stars the effective thickness of the galaxy can thus be taken as about 3,000 light-years, except in the region of the galactic centre where it appears to be swollen out to some three or four times that thickness. On the other hand, as has been repeatedly mentioned, there are certain comparatively small classes of stars which like the globular clusters show little or no tendency to share in this property of the majority to concentrate near the galactic plane—and these classes include those of the long period variables and of the shorter period Cepheid variables. It was with these latter stars that Professor Shapley was now concerned in his attempt to determine the *maximum* thickness of the galaxy as opposed to the thickness of the highly condensed galactic disc formed by the majority of stars. A search was therefore made in sample regions of the sky fairly near to the galactic poles for isolated short period Cepheids. By 1934 fifty-four stars of this type—including over forty which had not previously been detected—had been studied in sufficient detail to give their distances. Of these, eighteen were found to be more than 30,000 light-years above or below the galactic plane, while two on the northern side of the plane were at the immense distance from it of over 60,000 light-years.

From this it might be supposed—and indeed the suggestion has been made—that these shorter period Cepheids do not really belong to our galaxy, but are distributed throughout the vast intergalactic spaces that separate the spiral nebulae. Against this view, however, Professor Shapley was able to bring strong evidence; for he found that although there were quite a number of these stars in the directions of the galactic poles they were considerably more numerous in the directions of the galactic plane. In other words, they showed sufficient evidence of galactic concentration to make it quite clear that they were members of the galactic system. It follows that though the vast majority of the stars of the stellar system are confined within a shallow disc, whose thickness, except at its centre, is about 3,000 light years, a small minority of stars are sparsely scattered to enormously greater distances on either side of that disc and thus give to the system a *maximum* thickness round 60,000 or even 100,000 light-years. Now this is practically the same figure as is accepted for the diameter of the galaxy in the galactic plane—an effective diameter of 90,000 light-years plus possibly an extra 30,000 light-years to include Professor Shapley's occasional Cepheid stragglers at greater distances. Thus the highly flattened stellar system—the conception of which dates back to William Herschel—is now found to form the equatorial plane of an approximately spherical enveloping system composed not only of the globular clusters but also of a very sparse sprinkling of stars.

Practically at the same time as our own galaxy was being extended in thickness, a similar extension was being brought about in the Andromeda nebula. For the new Harvard technique for measuring the extent of the photographic images of nebulae, which had about doubled the longest diameter of the Andromeda nebula, was found to increase its shortest diameter some five or six times. Since this nebula is tilted away from us in such a way as to appear very considerably foreshortened, this increase in its shortest diameter will be due mainly to an extension of the system above and below the flattened and condensed spiral disc which had previously been regarded as representing the whole of the nebula. The results indicated that, as in the case of our own galaxy, the Andromeda nebula consisted of a highly flattened disc-like system imbedded in a much more diffuse envelope of stars which was approximately spherical.

We must now compare our galaxy and the Andromeda nebula with the external galaxies in general; but first we must consider the various types that are found among the external galaxies and their classification. It has long been known that many of the extra-galactic nebulae do not exhibit the characteristic spiral shape. Undoubtedly this is often due to their remoteness: either the photographic images are too small to reveal their true structure, or they are so faint that only their central portions show up. Thus in 1914 J. A. Hardcastle found that of some 700 extra-galactic nebulae appearing on the photographic charts of the heavens taken by Franklin-Adams only one-quarter showed a definitely spiral form. A much lower proportion, to wit, only one per cent., was obtained by Professor Shapley in 1924 from an examination of nearly 3,000 extra-galactic nebulae catalogued at Harvard. The average distance of this second and larger sample was necessarily very much greater—hence the difference between the two results.

Nevertheless such variations in the appearances of the extra-galactic nebulae could not entirely be ascribed to differences in their distances or to differences in the powers of the cameras employed. In 1926 a study was made at Harvard of 167 extra-galactic nebulae which were members of one of the super-galaxies in Virgo. Since all the nebulae in this group are at the same distance from us they are photographed, so to speak, to the same advantage; any variety of shape found among them must therefore be real rather than apparent. In this particular group approximately half the number turned out to have a spiral form; but it did not necessarily follow that the same proportions would be found in other super-galaxies or among the extra-galactic nebulae in general. The chief conclusion to be drawn from this investigation was that among the extra-galactic nebulae a considerable proportion were definitely not spiral in form. That same year Professor Hubble published the results of a similar study made of the photographs taken with the 100-inch telescope. Naturally this much larger instrument brought many more nebulae—whether single nebulae or members of super-galaxies—within reach of a critical decision as to whether they were spiral or not. Consequently the results obtained give a much fairer idea of the relative proportions of these two types among the extra-galactic nebulae in general. He found seventy-four per cent. to be spirals, three per cent. to be quite irregular in shape, and twenty-three per cent. to be elliptical in outline.

The question will probably be asked as to what the evidence is for including these irregular and elliptical nebulae in the same category as the spiral nebulae. That they are definitely extragalactic is shown by their distances and dimensions which are in every way comparable with those of the ordinary spirals. Again, like the spirals, the redward-shifts of their spectral lines indicate enormous velocities of recession. Finally, in every super-galaxy there is that constant association of spiral and non-spiral forms.

Many authorities have worked on the classification of the extragalactic nebulae and have divided them up into the three main classes, elliptical, spiral, and irregular. In the spiral class two main subdivisions are recognized. First, there are the " normal spirals " in which the two arms start curving away directly from opposite points on the edge of the nucleus. Secondly, there are the " barred spirals " in which the arms jut out as two straight bars from opposite sides of the nucleus and only take up their spiral curvature at some distance from the nucleus. The more detailed classification is due mainly to the work of Professor Hubble, who has succeeded in giving us a continuous sequence of types each one of which merges smoothly into the next.

The elliptical nebulae are entirely structureless and show no signs of resolution into stars, or into knots or condensations of greater brightness. They are brightest in the centre and fade off regularly towards the periphery. Their images show every degree of elongation, their outlines ranging from perfect circles, through slightly and highly elongated ellipses, to a fully developed spindle-shaped contour with drawn-out, pointed extremities.

The spirals, both normal and barred, are put into three classes. In (*a*) the arms are closely coiled about the nucleus, neither the nucleus nor the arms show any sign of resolution into condensations or stars, and the nucleus forms a relatively large part of the nebula. In (*b*) the arms are more widely open, condensations are beginning to appear in the outer portions of the arms, and the arms have increased in bulk at the expense of the nucleus. In (*c*) the arms are very loosely wound and are broken up into condensations throughout their entire length, the unresolved central nucleus now contributes but a small part of the total bulk of the nebula.

In using this classification one again has to be careful to distinguish between *appearance* and *reality*; and this is particularly difficult in the case of the different types of elliptical nebulae. In the spiral types the extent to which the arms appear uncoiled will clearly depend on the angle at which the nebula is tilted towards us. But since the spirals are known to have the shape of a flattened circular disc, the extent to which they *appear* elongated will always give us a measure of the amount of foreshortening due to tilt. Again, the resolution of the arms into condensations must depend on the distance of the nebula and on the size of the instrument used; but so long as we confine the classification to nebulae that are within easy reach of the instrument this difficulty does not arise. The matter, however, is much more serious in the case of the elliptical nebulae. It is *a priori* possible that the elliptical nebulae are all similar in shape to the unresolved nuclear portions of the spiral nebulae; that they are all flattened circular discs; that their circularity or ellipticity is simply a question of foreshortening. The other possibility is that some of these nebulae are really globular in shape, that others are bun-shaped with a greater or less degree of flattening, that yet others are lens-shaped, like a double-convex lens with a sharp surrounding rim. Although in the case of any particular nebula it is impossible to decide between these two alternatives, one can from a statistical study conclude quite definitely whether the elliptical nebulae are all circular discs variously foreshortened, or whether they are really sometimes globular, sometimes bun-shaped and sometimes lens-shaped. It is reasonable to assume that the orientation of the nebulae in space is entirely haphazard, that the amount of foreshortening due to tilt has a completely random distribution. If, therefore, all the nebulae are flat or very thin lens-shaped circular discs, one can calculate on that assumption what proportion in a large number of nebulae would appear (1) broad-side on, or circular, (2) of various degrees of ellipticity, and (3) edge-on, or either linear or thinly lens-shaped. These proportions can then be compared with those actually obtained from the measurements of large numbers of nebular images on photographs. In this way it has been shown that the elliptical nebulae cannot all be flattened circular or thin lens-shaped discs variously foreshortened; and that, while many of them do possess this shape, some must be bun-shaped with various degrees of elongation and others must be practically spherical.

One can thus arrange the extra-galactic nebulae in the following sequence. The first type is the spherical nebula. Next comes the bun-shaped type with a gradually increasing amount of flattening. This is followed by the lens-shaped type in which the equator, instead of being rounded off as in the previous type, is sharpened to a fine edge. In the next stage the equatorial edge becomes drawn out into a thin encircling rim, so that the nebula when viewed edgewise appears spindle-shaped. According to Dr. Hubble this last stage passes continuously into the first stage of the " barred spirals," but is separated by a gap from the first stage of the " normal spirals." In the earliest of the barred spirals the two arms together form one continuous ring round the nucleus, but in the earliest of the normal spirals so far detected the spiral character of the arms is already fully developed. The gap, however, does not necessarily vitiate the significance of the sequence; it may simply mean that the intervening stages are rare, or if the sequence is an evolutionary one that such stages are passed through very rapidly. It seems probable therefore that the sequence bifurcates after the spindle-shaped stage into the two sequences of barred and normal spirals. We have already seen how each of the spiral types proceeds through stages in which the arms open up and develop at the expense of the nucleus, while condensations, appearing first in the extremities of the arms, gradually spread inwards until they are found throughout the entire nebula. Finally, there are the irregular nebulae which as their name implies exhibit neither spiral structure nor symmetry of outline. As regards their contents, the majority of the irregular nebulae appear most closely related to the last spiral stage, for their resolution into condensations is very complete and central nuclei are either inconspicuous or absent. For these reasons some people have placed them at the extreme end of the sequence just given, suggesting that they represent the completely resolved and disintegrated spiral. Others regard them as representing exceptional offshoots from any one of the various stages in the sequence, and thus consign them without further ado to a nondescript class of abnormal types. But in view of their rarity—for they comprise less than three per cent. of the extra-galactic nebulae—it is best for the present to leave quite open the question of their relationship to the rest of the sequence.

In the discussion of the distances of the extra-galactic nebulae, we saw that Dr. Hubble had found that these bodies were all very nearly of the same absolute brightness. The average absolute brightness is about eighty-five million times that of the sun; and though one may occasionally come across a nebula that is considerably brighter or fainter than this, about half of them lie within the narrow limits set by double this figure on the one hand and by half this figure on the other. Yet it was still possible that the absolute brightness might depend upon the *type* of the nebula, that the absolutely faintest nebulae might turn out to be of one type and the absolutely brightest of another type. Of this, however, Dr. Hubble found no indication; and he concluded that the average absolute brightness of each type of extra-galactic nebula was appreciably the same.

But though there is no difference in the average absolute brightness of the various types of nebulae, the studies of Mr. Reynolds in this country and of Dr. Hubble and Professor Shapley in America have revealed considerable differences in their average absolute dimensions: the elliptical nebulae being definitely smaller than the spirals. Dr. Hubble finds a fairly regular progression in size as one passes along the sequence, the globular nebulae having an average absolute diameter of about one-fifth of that of the late type spirals. Professor Shapley, however, using the self-recording photometer for measuring what we may term the " over-all " as opposed to the " effective " diameter, finds that the elliptical nebulae are only about twenty-five per cent. smaller on the average than the spirals. So it would seem that whereas the outer rarefied envelope of stars increases but little in size throughout the sequence, the central and much more condensed system undergoes a considerable and progressive expansion. The irregular nebulae which are less constant in size have an average diameter about equal to that of the early type spirals.

If the average absolute brightness of the nebulae remains constant throughout the sequence while the average absolute size progressively increases, it follows that the surface-brightness of the nebulae (the intensity with which their surfaces shine) must progressively diminish through the sequence. For example, if an average-sized globular nebula and an average-sized late type spiral are at the same distance from us, they will appear of the same total brightness; but the globular nebula being considerably the smaller will have the more intensely luminous surface. Now Dr. Hubble has shown that the surface-brightness is remarkably constant for each type: it diminishes continuously, as we have seen, from one type to the next, but for all nebulae of any particular type it is the same no matter whether they be above or below the average in total brightness and size. A set of globular nebulae including those that are abnormally large and bright, or abnormally small and faint, will all have the same surface-brightness. And a set of late type spirals including those that are abnormally large and bright, or abnormally small and faint, will again all have the same surface-brightness. But the surface-brightness of the first set will be considerably greater than that of the second set.

These relations between the form, the size, the total brightness and the surface brightness of the nebulae strongly suggest that the types in the sequence represent stages in the gradual expansion of a globular system of stars into a spiral system of stars. For if a system of stars were to expand in this way its total brightness would remain unchanged, while the brightness of each small portion of it would diminish as its size increased. That would of course imply that the sequence is an evolutionary one and represents the stages through which the extra-galactic nebulae develop. We shall now see what other evidence there may be to support such a view.

The question naturally arises why one can make out some of the individual stars in the later type spirals and irregular nebulae while it is impossible to detect any stars in the globular and elliptical nebulae, in the earlier spirals and in the central nuclei of the later spirals. In these the appearance is that of an unresolved mist quite unbroken by any star-like points of light. One suggestion is that in these cases the stars are enveloped in a vast cloud of finely divided dust, which, acting rather like a ground-glass screen, diffuses the light of the individual stars that illuminate it into a hazy blur. The other explanation is that stars bright enough to be seen individually at such enormous distances only exist in the irregular nebulae and in the outer resolved portions of the later type spirals. Dr. Hubble has tentatively put forward the suggestion that this may again be a matter of evolutionary development: that the brightest stars, which are mostly white giants, only begin to appear when a galaxy is in its latest developmental stages.

We have already seen that the extra-galactic nebulae give the spectrum of integrated star-light. The actual type of spectrum is closely similar to that of the sun.[1] This is what we should expect; because it is probable that if our own galaxy could be observed from a distant point in space the integrated light of all the stars in it would also give a solar type spectrum. In a few of the late spirals and irregular nebulae one finds superposed on the absorption spectrum of star-light the bright lines characteristic of gaseous nebulae. But this does not weaken our contention that the extra-galactic nebulae are essentially stellar systems: the bright lines are simply due to the presence in the larger system of gaseous nebulae similar to those scattered about our own galaxy. In fact in most cases the bright lines can be localized to isolated patches of light within the spiral which are undoubtedly analogous to the diffuse galactic nebulae.

Although the spectra of the extra-galactic nebulae are remarkably constant from one to another, it has been shown by Dr. Humason that there is a slight but definitely progressive change as one passes along the sequence. The globular and elliptical nebulae give the spectrum of a G4 star, a star slightly yellower than the sun; while at the other extreme the late type spirals give the spectrum of an F9 star, a star slightly whiter than the sun. This progression, which appears to be well-established, has been confirmed in the last few years by Professor Stebbins' direct colour measurements. Dr. Humason's spectral types were obtained from a study of the absorption lines; Professor Stebbins' results were got by measuring the brightness of the nebulae through colour-filters with a photo-electric cell. This last method also showed that there was a slight gradual falling off in the redness of the nebulae as one passes from the elliptical types to the late spiral types. Now already in 1922 Dr. Seares of Mount Wilson had found that the outer portions of the arms of late spirals were considerably less red than their unresolved nuclei. It now seems probable that the reason for this is, that it is in the late type spirals, and particularly in their outer parts, that the bright, early type white giant stars are to be found.

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[1] The solar-type spectrum of the Andromeda nebula was first noted by Scheiner in 1899, and confirmed in greater detail by Slipher in 1912.

Although all these points—the gradual expansion, the increasing resolution with the appearance of white giant stars, the diminishing redness and the progression in spectral type—may be taken as suggesting that the sequence of the nebulae is an evolutionary one, they do not in any way prove that it is so. Our knowledge of the stellar constituents of the extra-galactic nebulae is at present confined to a very small sample of the nearer of them, and even in these we know only about the brightest of the constituent stars, giants and super-giants which form but a tiny minority among the stars in general. But the suggestion is an interesting one; and if it should turn out to be correct, the telescopes of the future—the 200-inch and those that come after it—by bringing within our reach still fainter stars in these nebulae, may be expected to throw a new light not only on the evolution of the galaxies but on stellar evolution in general.

There are five extra-galactic nebulae that are approximately at the same distance from us as the Andromeda nebula and two others that are very much nearer to us. Since this small group happens to comprise examples of practically every type of nebula in our sequence, it will be best to consider them in some detail before drawing our final conclusions as to the exact status of our galaxy among the extra-galactic nebulae. The above statement that there are two extra-galactic nebulae much nearer to us than the Andromeda nebula will probably come as a surprise to the reader. The truth is that it is only during the last few years that the extra-galactic status of these two nebulae, which are the two Magellanic Clouds, has come to be recognized. They were previously looked upon as detached outlying portions of our own galaxy. But since we have come to regard that rare class of object, the irregular extra-galactic nebula, as belonging to the same category as the other extra-galactic nebulae, it has been realized that in the Clouds of Magellan we have in our immediate vicinity two typical examples of these irregular nebulae.

We have repeatedly referred to the tendency of the extra-galactic nebulae to congregate together into groups. In this respect they behave very much in the same way as ordinary stars. Thus just as we get single, double, triple or multiple stars, so we get single, double, triple or multiple nebulae; while corresponding to the star-clusters we find clusters of nebulae, or super-galaxies as they are called, which generally contain several hundred members. It seems that our own galaxy is a case in point and is a member of a multiple galaxy with at least nine components. We say " at least" because there may be others lying in the direction of the Milky Way and hidden from view by its obscuring clouds. In fact Dr. Hubble has recently suggested three such nebulae as possible candidates, but owing to the density of the obscuring material through which they are observed their distances from us cannot be judged with any degree of certainty. We shall therefore confine our survey to the nine nebulae, including our own galaxy and the Andromeda nebula, which are known to belong to the group.

The group contains two spirals and probably three. The Andromeda nebula is a spiral of intermediate type, for while the outer whorls are well-resolved into condensations and stars, it possesses a relatively large unresolved nucleus. The second spiral is the nebula M33 in the constellation of Triangulum. This is a late type spiral, all of it except a small unresolved nucleus being broken up into condensations and individual stars. The third is our own galaxy which is *probably* also a late type spiral very similar in structure to M33.

The two elliptical nebulae in the group are close neighbours of the Andromeda nebula. Like other elliptical nebulae they show no signs of resolution into condensations or stars; and since they are at approximately the same distance as the Andromeda nebula, this constitutes a definite proof either that no very bright stars like Cepheids or white giants exist in them, or that such stars are obscured from view by enveloping clouds of dust or gas.

The four remaining nebulae in the group are oddly enough of the rare irregular type. The two nearest of these, the Magellanic Clouds, form two close companions to our galaxy analogous to the two elliptical companions to the Andromeda nebula. Owing to their proximity—about one-seventh of the distance of the Andromeda nebula—we know far more about their stellar content than about that of any other extra-galactic system. The two other irregular nebulae are at distances comparable with that of the Andromeda nebula. All four are typical of the irregular nebulae in their utter lack of symmetrical form and in the high degree of resolution extending to their centres.

The nebulae of this our local group, even though they are by far the nearest of all nebulae, are so remote that a star equal in brightness to the sun situated within one of them would be a very long way beyond the reach of the 100-inch telescope. Unfortunately the Magellanic Clouds, the two nearest members of the group, are so close to the south pole of the heavens that they cannot be studied with the 100-inch telescope which stands well north of the equator. All the other members, however, can be photographed with the 100-inch; yet any stars in them in order to be identified individually on the photographs must be at least several hundred times as bright as the sun. The only stars which (at least photographically) are as bright as this are the Cepheids, the white giants, a very occasional yellow or red giant, and the "new stars." Our knowledge of the Magellanic Clouds has been derived almost entirely from the elaborate photographic surveys of them carried out at the Harvard stations in the southern hemisphere—particularly in recent years at Bloemfontein. The camera chiefly used has a lens two feet in diameter, and though this is a very large instrument it is small in comparison with the 100-inch mirror. But the nearness of the clouds more than compensates for the difference in size of instrument, and the Harvard observers can reach any star in the clouds that is not less than 100 times as bright as the sun.

We shall now enumerate briefly the contents of the various nebulae in our local group, following for convenience the sequence of our classification.

(1) The two elliptical nebulae as we have said contain no stars that are detectable individually. They appear as completely unbroken patches of hazy light. It follows that in them there can be no stars of the brightest types unless such stars are obscured by a dusty envelope. Their spectra moreover show no bright emission lines, so there is no evidence of the existence in them of gaseous nebulae.

(2) The spiral group contains no example of an early type spiral, so we pass at once to the Andromeda nebula which is of intermediate type. In the outer resolved parts of this spiral, Cepheids and white giants are found in considerable numbers, while numerous novae—115 in all—have been observed during their apparitions. One red super-giant is known similar to our galactic Betelgeuse, the great star in the shoulder of Orion; and the resemblance is emphasized by both stars being irregular variables. There is one large star cloud similar to those of the Milky Way, and a few open clusters; while on the outskirts of the nebula there are about 150 globular clusters. The relative proportions of the different types of the brightest stars are approximately similar to those in our own galaxy; but a striking point of difference is that so far no gaseous nebulae have been detected in the Andromeda nebula.

(3) The late spiral type is represented by M33 and probably by our own galaxy. The spiral arms of M33 are resolved well-in towards the centre of the nebula into stars, star-clusters and star-clouds. The frequency of open clusters and clouds makes this nebula much more akin to our own galaxy than to the Andromeda nebula. Numerous Cepheids are found and particularly large numbers of white giants. Again, the great preponderance of white giants is more akin to the galaxy than to the Andromeda nebula. A few novae have been observed and a few globular clusters detected. Another striking feature that emphasizes the resemblance to the galaxy rather than to the Andromeda nebula is the existence of numerous patches of gaseous nebulosity giving bright line spectra and numerous patches of obscuration similar to the dark galactic nebulae. Moreover as in the Milky Way the bright gaseous nebulae are all associated with bright white giants; and, according to Dr. Hubble, the same relation holds between the size of the nebulae and the brightness of the involved stars as holds in our own stellar system.

The evidence for our own galaxy being a late-type spiral is of an indirect nature. It is not possible, for example, to map out the distribution of the Milky Way star clouds and show that they outline a system of spiral shape. We are however practically certain that the galaxy belongs to the same category as the extra-galactic nebulae; it cannot be an elliptical nebula because of its high degree of flattening, and it cannot be an irregular nebula because of its marked rotational symmetry; presumably therefore it is a spiral. Finally, because of the way in which it is broken up into star-clouds and open clusters, and because of the large numbers of white giants and gaseous nebulae, one naturally places it among the later type spirals such as the nebula M33 we have just considered.

(4) The Magellanic Clouds appear to the naked-eye as two large hazy patches of light, and resemble as we have said two detached fragments of the Milky Way. They show no rotational symmetry and no suggestion of spiral structure: in fact they are typical irregular nebulae. With the instruments of the Harvard station at Bloemfontein their stellar contents have been studied in great detail down to a limiting brightness somewhat less than 100 times that of the sun. Over 3,000 Cepheids have been discovered in the two clouds, and a certain number of long period variables, irregular variables and eclipsing variables have been recognized. Open clusters and gaseous nebulae are found in great abundance; and white giants, including many of unusually high luminosity, are particularly numerous. This richness in white super-giants is specially noteworthy in the larger of the two clouds; and there also is found the brightest of all known stars, S Doradus, an irregular variable star with a peculiar bright and dark line spectrum some 350,000 times brighter than the sun. Finally, dark obscuring nebulosities and globular clusters occur in both clouds, while one nova has been observed in each of them. The stellar contents of these clouds down to the observable limits are very similar to those of our own galaxy; and this is particularly well illustrated by the Cepheids in the Large Cloud, the relative numbers of which between different limits of absolute brightness are almost identical with those of the galactic Cepheids. The chief difference in content between the Clouds and the galaxy lies in the great frequency of Magellanic white giants and in their high luminosity.

The two other irregular nebulae of the group are much more remote, being at distances comparable with that of the Andromeda nebula. Their absolute dimensions are considerably less than those of even the Small Cloud. Except for the absence of white giants of abnormally high luminosity, their stellar contents are very similar to those of the two Clouds.

If we could regard these four irregular nebulae as typical of the class in general, we should undoubtedly relate them most closely to the class of late type spiral; and if further we could consider the sequence to be an evolutionary one, we should reasonably look upon the irregular nebula as the last state of the disintegrated spiral. We should do this mainly because of the large numbers of gaseous nebulae and early type stars observed in them. Unfortunately what we know of the irregular nebulae outside our local group, which because of their rarity is comparatively little, suggests that they do not belong to a homogeneous class and are not all built on the same plan as the four in our vicinity. We cannot, therefore, make use of the Magellanic Clouds to settle the question of the status of the irregular nebulae as a whole. The unique value of the study of these two clouds is that it gives us a bird's-eye view of two extra-galactic systems whose contents in general outline closely resemble those of our own galaxy and of the later type spirals. With the spiral nebulae the distance is so great that most of the detail in the view is obliterated; with our own galaxy our situation within it prevents our seeing the wood for the trees; but with the Clouds of Magellan, at a distance large but not too large, all the details in the view are laid out to equal advantage—like the sky-line of New York as one sails into harbour—no part of it unduly prominent and no part of it dwarfed by greater distance. Already the Harvard studies of these two nearby galaxies have given us a much clearer insight into the real relative abundance of the different kinds of the brightest stars. And when in a few years' time the Radcliffe Observatory at Pretoria sets the example by establishing the first great reflecting telescope in the southern hemisphere, this inestimably valuable bird's-eye view will no longer be limited to stars more than a hundred times brighter than the sun but will begin to be extended so as to include the fainter and more common varieties of stars.

We need have little hesitation in assuming that the contents of the more distant of the extra-galactic nebulae are generally similar to those of the nebulae of corresponding types in our local group—with the possible exception of certain of the irregular nebulae. In support of this is the striking structural similarity of all the nebulae of any particular type so vividly brought home to one by the photographs. But in the case of some of the nearer of these remote galaxies there is additional evidence; for there are over one hundred of them that are still close enough for the 100-inch to reveal within them their brightest stars and star-clusters.

Though the Cepheids are among the brightest of the stars in the galaxies of our own local group there are always a small number of other stars and star-clusters that are yet brighter: certain irregular variables, novae at their maxima, white giants, globular and open clusters, and super-novae. It seems that globular and open clusters are usually about as bright as the brightest white giants, while these in their turn are somewhat brighter than the average nova at its maximum and the brightest irregular variables. Super-novae are of course very much brighter than any of the above, but they are so extremely rare that they can be discounted. There is, however, one other kind of celestial object that is detectable at these enormous distances, and that is the gaseous nebula. For though the gaseous nebulae do not compare in luminosity with the brightest stars, they have the advantage that the whole of their light is concentrated in a few spectral lines instead of being spread out over the whole range of the spectrum. It is for this reason that their presence can often be detected spectroscopically in nebulae which are so remote that only a few of their brightest stars can be directly photographed.

From a study of the 125 nearest of the later type spirals outside our local group, Dr. Hubble has shown that the above-mentioned kinds of brightest stars and star-clusters fade out in order of increasing brightness as one penetrates to greater and greater distances. In the nearest of these nebulae an occasional Cepheid can still be found; in others rather more distant irregular variables and sometimes a nova can be detected; and finally, in the most distant of these 125 systems, only the brightest white giants, globular and open clusters and gaseous nebulae remain. From this investigation Dr. Hubble has been able to draw another conclusion of great importance; for he finds that on the average the three or four brightest stars or star-clusters in all later type spirals are approximately of the same absolute brightness: thus the apparent brightness of such objects furnishes him with yet another criterion of the distance of the galaxy in which they are situated.

It still remains to be seen how our galaxy compares in size with the extra-galactic nebulae in general; for so far we have compared the dimensions of the stellar system only with those of the Andromeda nebula. In that comparison our final conclusion was that there was probably very little to choose between the two. We found the diameter of 40,000 light-years for the main mass of the Andromeda nebula to be about doubled when its envelope of sparsely scattered stars and its outlying globular clusters were included; and we decided that 90,000 light-years was the most probable value for the corresponding over-all diameter of our own galaxy. Yet there is still the possibility that the Andromeda nebula is itself an exceptionally large galaxy; and we must now see whether this is or is not so.

We saw that Dr. Hubble had shown that the average size of the various types of extra-galactic nebulae increased as one passed down the sequence, the late spirals having about five times the diameter of the globular nebulae. The actual dimensions of a large number of extra-galactic nebulae have been determined by the late Professor de Sitter, Professor Shapley and particularly at

Mount Wilson by Drs. Hubble and Humason. From these numerous measures Dr. Hubble gives the average diameter of the globular nebulae as about 2,000 light-years and that of the late type spirals as about 10,000 light-years—the elliptical nebulae, the earlier spirals and the irregular nebulae being of intermediate sizes. These figures refer to the effective diameters or the diameters of the more condensed parts of the respective types, and should therefore be approximately doubled to give the corresponding over-all diameters. Yet even so they obviously fall very far short of the diameters found above for the galaxy and the Andromeda nebula. Dr. Hubble's figures, however, give only the average diameter for each type; and though the majority of the nebulae conform closely to the average, there are exceptional ones that may be very much larger or smaller. This has been well shown by Professor Shapley's study of the sizes of the various nebulae in some of the nearer of the super-galaxies. The super-galaxies are practically alone suited to this purpose. Their distances can be measured with considerable accuracy, and within them there are collected together a large assortment of nebulae which we know definitely are all at the same distance from us. The absolute dimensions of the individual nebulae in such an assortment are therefore not subject to the uncertainty that a *large nebula may look large* simply because it is comparatively close to us. On the other hand the distances and dimensions of isolated nebulae, which are too remote for their brightest stars to be detected, can only be obtained by statistical methods that depend essentially on the assumption that their dimensions approximate to the average. And that obviously implies that such methods are not valid for the *exceptionally* large nebulae. Professor Shapley found that in a super-galaxy in the Great Bear the largest members had diameters of 20,000 light years. In the super-galaxies in Virgo and Coma Berenices he got diameters for the largest spirals running up to 24,000 and 25,000 light-years, while two of the elliptical nebulae had diameters of 15,000 and 18,000 light-years respectively. Finally, in a super-galaxy in Centaurus the diameters of the largest spirals reached 45,000 light-years. So when we remember that these figures represent *effective* diameters, and should probably be doubled to give over-all diameters, we may conclude that while our stellar system and the Andromeda nebula undoubtedly have unusually large dimensions they are by no means unique among the galaxies. Professor Curtis believes that the extra-galactic nebulae tend to fall into one of two groups: the majority approximate to the average dimensions found by Dr. Hubble, while a minority form a class of " giant galaxies " with diameters ranging from about 15,000 light-years to 50,000 light-years or more.

Most of the members of our local group conform closely to the standard sizes of the various types. The late spiral M33 with a diameter of 12,000 light-years can be regarded as an ordinary galaxy slightly larger than the average of its type which has a diameter of 10,000 light-years. The two elliptical nebulae, on the other hand, are below the average in size. One has a diameter of 1,500 light-years, the other a diameter of only about 800 light years—the average for the type being about 3,000 light-years. The average diameter of the irregular nebulae is given by Dr. Hubble as about 6,000 light-years. The diameter of the Small Magellanic Cloud is practically identical with this figure; but the Large Cloud with its diameter of 12,000 light-years may perhaps be regarded as belonging to the class of giant galaxies of irregular type. The remaining two irregular nebulae are somewhat below the average in size with diameters around 3,000 and 4,000 light-years respectively. So our local group, containing among its nine members two giant spirals and one abnormally large irregular nebula, makes up in quality what it lacks in quantity.

Thus we conclude our comparison of the stellar system with the extra-galactic nebulae. In doing so we may say that recent studies have made it no longer necessary to look upon our galaxy as a " continent " unique in the midst of myriads of lesser " islands." Certainly it is a giant among the galaxies; but as such it does not stand alone as did Gulliver in the land of Lilliput. Rather it comes of a race of giants, such, as we are told, once on the earth held sway among men.

CHAPTER XXIII

THE EVOLUTION OF CELESTIAL SYSTEMS

WE have already dealt in various parts of this book with some of the problems relating to the evolution of the heavenly bodies: the evolution of the stars themselves as inferred from a study of the sequence of spectral types; the evolutionary status of the various types of galactic nebulae; and the cataclysmic formation of novae. There remains, however, a class of problem of a somewhat different kind which is concerned with the formation and evolution of the various systems found among the heavenly bodies: the spiral systems exhibited by the majority of the extra-galactic nebulae, the systems of the open and globular clusters, the systems of the multiple stars and the double-star systems, and, finally, the solar system itself with the sub-systems of its planets and their satellites. The problem as to how these various configurations came about is clearly of the most fundamental interest to the astronomer. Unfortunately, however, it can only be tackled in a purely theoretical manner while the solutions obtained can only be tested very indirectly by observation. In this book we have considered primarily the observational aspects of astronomy and avoided theoretical discussions except in so far as they bore directly upon the observations. We shall therefore give but the briefest outline of this highly theoretical subject. The theories as to the manner of formation of the various types of celestial systems have been partly speculative and partly based upon critical mathematical reasoning. In many of the earlier theories speculation predominated; but in recent times theories which failed to stand up to rigid mathematical criticism have seldom passed beyond the stage of the author's manuscript. For this reason reference will be made only to those earlier theories which have stood the test of time or which have played an important part in the development of current theories.

The original "Nebular Hypothesis" was independently formulated in the latter half of the eighteenth century by the philosopher Kant and the mathematician Laplace, in order to account for the origin of the solar system. About the same period the elder Herschel developed a somewhat similar theory in connexion with the observations made with his great telescopes, in which he supposed the nebulae to be the birthplaces of the stars and the forerunners of the star-clusters. But it was only Laplace who founded his theory on a definite mathematical basis. He supposed that in the distant past all the material in the solar system was in the form of a flattened mass of diffuse gas which extended beyond the present confines of the system, was intensely hot and was slowly rotating. As the nebula contracted under the influence of its own gravitation its speed of rotation would increase until the centrifugal force at its periphery became greater than the gravitational force. Laplace supposed that this would result in a ring of gas becoming separated from the nebula all round its edge, while the main bulk of the nebula would go on contracting. This process, it was supposed, would be repeated periodically until one was left with a comparatively small central sun surrounded by a series of concentric rings. Meanwhile each ring would break up into fragments, which would gradually conglomerate to form a planet revolving in an orbit previously outlined by the corresponding ring. It was further supposed that each planet while still gaseous might go through the same process as the original nebula, though on a smaller scale, and give rise to a system of satellites. It was suggested that this would explain the rings of Saturn, which for some reason or other had not broken up in the normal manner into satellites.

The theory accounted for many of the remarkable uniformities which are so outstanding in the solar system. It explained why the paths of the planets are so nearly circular and so nearly in one plane, and why the planets all revolve in the same direction round the sun. Since the rings after their separation from the nebula were supposed to rotate as *solid* rings—their outer edges moving faster than their inner edges—the theory also explained why the planets rotated on their axes, and the satellites revolved, in the same direction as the planets themselves moved round the sun. For at that time the retrograde rotation of Uranus had not been ascertained, Neptune had not been discovered, and the only satellites known to revolve in the contrary direction were the moons of Uranus.

Weak points gradually began to appear in the theory as the result of fresh astronomical discoveries and of more detailed mathematical analysis. Observations of the system of Uranus and of the more recently discovered system of Neptune showed that the rotations of these planets and the revolutions of their satellites were all in the retrograde direction. A still greater objection came with the discoveries of an outer satellite of Saturn and two outer satellites of Jupiter which not only travelled round their parent planets in the backward direction but did so in spite of the fact that the rotation of these two planets and the revolution of their other satellites were in the normal direction. Another difficulty arose with the discovery of the two satellites of Mars, for the inner one revolved round the planet in a shorter period than Mars itself rotated on its axis. According to the theory such a state of affairs was impossible. The speed with which a satellite is revolving must approximate to that with which the planet was rotating when previously swollen to the diameter of the satellite's orbit. Since, after the separation of the satellite from the planet, the latter must continually shrink and speed up its rotation, the planet must always rotate more rapidly than its satellite revolves.

The objections to the theory on mathematical grounds were even stronger. In his famous essay on the stability of Saturn's rings, published in 1857, Clerk Maxwell proved that if the rings were solid or even liquid structures they would immediately collapse, and that their permanence implied that they were composed of myriads of discrete particles revolving independently of one another. Thus the idea that the rings of Saturn represented an abnormal hold-up at the half-way stage in the development of one or more satellites had to be abandoned. Later George Darwin (son of the great biologist Charles Darwin) showed that if a ring did form and condense into a planet, that planet would be situated not on the periphery of the ring but at its centre: the separated material would in fact fall back again into the nebula. Still more recently Sir James Jeans has pointed out that such a ring if formed would probably not condense at all; the amount of material in it would be too small to cause it to condense under the influence of its own gravitation: instead it would simply diffuse away through space. Another serious objection to the theory was raised by Babinet in 1861. There is a well-known principle in physics called the *conservation of angular momentum*, which means that in any isolated system like the solar system the momentum due to the rotations and revolutions of the bodies within it must for ever remain constant; and it makes no difference whether the system has always consisted of discrete bodies as at present, or was at one time an undifferentiated mass of gas. Making use of this principle Babinet was able to show that unless one made certain very improbable assumptions as to the distribution of the material within the original nebula, the rotation could never have been rapid enough to bring about the detachment of the various planetary rings.

Though Laplace's Nebular Hypothesis as to the origin of the solar system has had to be abandoned in the light of more recent developments in mathematical and physical theory, it will always remain as a contribution to knowledge of fundamental importance. It was the first theory of its kind to be developed along strict mathematical lines; and though often severely criticized, it remained for over a century without any serious rival. By successfully explaining many of the peculiarities of our system and by failing to explain others it brought out into the light for the benefit of future investigators the numerous difficulties with which the subject is beset. But most remarkable of all is the fact that, although Laplace's theory fails in its attempt to explain the formation of the solar system, it is found to be almost identical in its main features with the current theory of the development of the extra-galactic nebulae. Sir James Jeans, to whom that theory is due, remarks that Laplace was wrong only as regards the *scale* of the system which he set out to account for. If Laplace's primitive nebula is but magnified some hundred million times it will pass through, according to current theory, almost in detail the series of stages prescribed in the original Nebular Hypothesis. To this subject we shall return presently.

In Laplace's theory, and in the various modifications of his theory put forward during the ensuing eighty years, the all-important mechanism was rotation. The rotation of the nebula increased until a point was reached when the balance between gravity and centrifugal force was upset in favour of the latter, with the consequent separation of the outer parts of the nebula. In 1879 George Darwin began the publication of a long series of investigations of quite a different mechanism, which was destined to play an equally important part in evolutionary problems. He termed this mechanism " tidal evolution," and investigated it primarily in connexion with the development of the earth-moon system.

Everyone is familiar with the ordinary tides at the sea-side. At most places there is a high-tide twice a day, while each day the time of high-water is on the average fifty-one minutes later than on the previous day. The tides are due to the gravitational attractions of the sun and moon; and since the moon, although the smaller body, is much the nearer of the two, the part which it plays in raising the tides is very much greater than that played by the sun. Now although the tide-raising forces of the sun and moon on the earth are *due* to their gravitational attractions, their tide-raising forces on the earth are by no means the *same* as their gravitational attractions on the earth. To understand this let us consider first the tides due to the moon alone. Since the gravitational attraction between two bodies diminishes very rapidly as the distance between them increases, it follows that the moon must attract the portions of the earth nearest to it more vigorously than it attracts the portions of the earth that are furthest away. And since the distance of the moon from the earth is only about thirty times greater than the diameter of the earth, the attraction exerted by the moon must vary quite considerably at different parts of the earth. As a result of the moon's attraction the earth tends to fall slightly towards it; but depending on their distance from the moon the different parts of the earth fall to different extents. The bulk of the earth being solid falls in one piece, just as though all parts of it were at the same distance from the moon and condensed into a small lump at the earth's centre. But the oceans being liquid behave differently. The parts of them on the side of the earth nearest the moon tend to fall towards the moon rather more than the earth as a whole; while the parts of them on the side of the earth furthest from the moon tend to fall towards the moon rather less than the earth as a whole. Consequently we get two regions of high-water on opposite sides of the earth, one directly facing the moon and one directly away from the moon: it is as though the first were drawn up by the moon, while the second were left behind by the earth. From this it will be seen that the moon's tide-raising force on the earth is not to be identified with its gravitational attraction on the earth, but rather with the *difference* in its gravitational attraction on different parts of the earth. We thus get an explanation of the curious anomaly that whereas the sun's gravitational attraction on the earth is nearly 200 times that of the moon, its tide-raising force on the earth is only about two-fifths that of the moon. For the distance of the sun from the earth is so enormous in comparison with the diameter of the earth, that its gravitational attraction on the nearer and on the further sides of the earth differs but very little.

The two tidal waves heaped up on the parts of the earth facing and opposite to the moon must, as a result of the earth's rotation on its axis, travel round and round the earth from east to west. Looked at in another way, they remain stationary in a line with the moon, while the earth rotates beneath them. Now since the moon is revolving round the earth in the same direction as the earth rotates, it will be overhead at a given place somewhat later each day. The difference in time is on the average fifty-one minutes per day, which it will be remembered is precisely the average daily difference in the time of high-water.

If there were no moon we should have only the much smaller tide due to the sun. But since the sun is overhead at a given place at approximately the same time every day, to wit at noon, the time of high-water instead of varying as at present would be always the same. As it is we have two sets of tides, one due to the moon and the other to the sun; and though the sun's tide is largely masked by that of the moon its effect is by no means inappreciable. When the two come together, as they do twice a month, at the new and full moon, when the sun and moon are both pulling in the same straight line, we get the specially high and low "spring tides." When the two occur in opposition to one another, the high-water of one coinciding with the low-water of the other—as happens at the moon's first and last quarters when sun and moon are pulling at right angles to one another—the larger lunar tide is partially counteracted by the smaller solar tide, and we get the so-called "neap tides."

It must be realized that the problem of the tides is much more intricate than the above explanation might lead one to suppose. The subject is greatly complicated by the interruption of the oceans by continents and islands as well as by other factors. For these various reasons the two tidal waves do not travel continuously round the earth, but are broken up in a most irregular fashion. Also the interval between the moon being overhead at a given place and the high-tide at that place, varies enormously and often most irregularly from one part of the earth to another. But such complications need not concern us in our consideration of the theory of tidal evolution. We shall regard the tidal waves as remaining more or less in a line with the moon and travelling continuously round the earth. And we must remember that there is also a tide in the solid earth similar to, though much smaller than, that in its oceans; for it has been shown that the earth is not perfectly rigid and "gives" slightly under the moon's pull.

Darwin first pointed out that, as a result of the tides, friction will occur between the water and the solid ground beneath it, which in the course of ages must produce an appreciable slowing up of the earth's rotation. Very roughly we may picture the process by regarding the tidal wave, which is stationary beneath the moon, as a sort of brake rubbing against the earth which is rotating beneath it. Actually, however, the friction is very slight in deep water and only becomes important in the shallows where large masses of water flow alternately in towards the land and out again to sea. The friction then is very considerable; and when the whole surface of the globe is taken into consideration, its production must involve the expenditure of a vast amount of energy. This expenditure is provided for from the energy of the earth's rotation; so the rotation must be slowing down, or, what comes to the same thing, the day must be growing longer. Theoretically this process must continue until the tidal wave has brought the rotating earth, relative to itself, to a standstill. That will be when the surface of the earth like the tidal wave remains stationary beneath the moon; when the earth always turns the same face to the moon, so that people on one side of the earth will always be able to see the moon, while people on the other side will never be able to see it; when, in fact, the day and the month are of the same length.

Now this is precisely the state of affairs that is found on the moon. For the moon keeps always the same face to the earth so that we have never seen the other side of it. This means that the earth shines always on one side of the moon and never on the other, and further that the period in which the moon rotates on its axis is identical with that in which it revolves round the earth. That this should just happen to be the case would obviously be an absurd coincidence; we can only deduce that there is a good reason behind it. And the only known reason is the one we have just discussed: in the same way as the moon now raises tides on the earth, so in the past the earth raised tides on the moon; but, because of the smaller size of the moon which would cause it to respond more rapidly to a given braking action, and because of the larger size of the earth which would give rise to bigger tides, the slowing down of the moon's rotation would have reached completion long before the slowing down of the earth's rotation.

But Darwin was able to do more than explain this coincidence of the moon's periods of rotation and revolution; he was able to deduce important information as to the past and future of the earth-moon system. He showed that this mutual slowing down of each other's rotations must result in the two bodies gradually pushing themselves further away from one another. This follows from the principle of the conservation of angular momentum previously referred to. If the angular momentum in the earth-moon system due to the rotations of the two bodies is diminished, the loss must be made up to the system by a corresponding increase in the angular momentum due to the revolution of the revolving body. And since the angular momentum of the revolving moon is increased by enlarging its orbit, it follows that the moon will gradually recede from the earth.

Though the effect is so slow that it cannot be detected there can be no doubt that this recession is now in progress, and will continue until the lunar tides have brought about the equality of our day and month and thus have terminated the tidal action between the two bodies.

Darwin deduced by the same reasoning that as one traced history backwards to earlier stages, when the mutual slowing down of the rotation periods of the two bodies had not progressed so far as it has now, the moon must have been revolving in an orbit nearer to the earth. The further one retreated into the past the faster and the nearer to the earth must the moon have been revolving, and the faster must both earth and moon have been spinning on their axes. Originally there must have been an epoch when the two bodies were revolving round their common centre of gravity almost in contact with one another and in a period of about four hours. Backwards beyond this point rigid mathematical reasoning could not go. But very tentatively Darwin suggested that in the stage immediately preceding this one the earth and moon formed a single body whose rotation was sufficiently rapid for it to break in two under the centrifugal force. Subsequently Darwin abandoned this last and admittedly tentative step in his argument; for he went on to prove that if ever the earth and moon formed a single body a rotation period of four hours would *not* have been rapid enough to bring about its division.

To do this Darwin approached the problem from the opposite direction. He assumed that at a certain epoch the earth and moon formed a single body still at a high temperature and in a plastic condition, having recently been born of the sun by some such method as pictured by Laplace's theory. He found that as this plastic mass contracted and rotated more rapidly it would lose its spherical shape, becoming first flattened at its poles and assuming later the form of a short and fat cigar. But though this was not yet the stage at which the break up of the body would have occurred automatically, it was nevertheless the point beyond which the speed of rotation had increased no further. But unless the speed of rotation *had* increased further, the plastic body would never have passed beyond the stable cigar-shaped form and would never have reached that unstable state which would have led to its division; and yet his previous argument, backwards from the present, had shown conclusively that this particular speed of rotation had never been exceeded. Thus the ends of these two lines of reasoning had failed to meet; between them there remained a gap which could only be bridged by assuming some extraneous process that had brought about prematurely the birth of the moon.

The extraneous process which Darwin now suggested was as follows. He pointed out that the *natural period of free vibration* of the plastic mass, which would change continuously as its contraction proceeded, would probably at some time have become equal to the period of the tides raised upon the body by the sun. When such equality was reached the quite small solar tides would have become enormously exaggerated, the body would have become distorted beyond breaking point and it would have burst asunder into two. In one way or another we are all familiar with this property of a body to respond in an exaggerated manner to an imparted vibration whose period is the same as its own natural period of free vibration. We have found it in caves which resound loudly when we sing a particular note. We make use of it in the "swing," when we give it a push at properly timed intervals, and succeed with the minimum amount of effort in sending it up to an alarming height. And all of us have at least heard of the great tenor who by singing the correct note could shatter into fragments a tumbler at the other end of the concert hall. This was the explanation that Darwin suggested for the premature birth of the moon. In 1917 Dr. Jeffreys of Cambridge, who recently received the gold medal of the Royal Astronomical Society for his work on the physics of the earth, concluded that there was considerable evidence in support of Darwin's theory. But in 1930 he took back this opinion and showed that the theory was untenable. Thus, although it is possible that the moon was born of the earth, the mechanism of its birth is unexplained.

At the beginning of the present century there was put forward by Chamberlin and Moulton of Chicago a theory of the origin of the solar system based primarily on the principle of tidal evolution. It supposed that originally the sun was a solitary star without planets; and that at some remote period in the past another star, journeying through space, happened to pass close by. After swinging around one another under their mutual gravitational attractions the two stars separated, the visiting star departing again into space. Meanwhile, during the encounter, the sun had given birth to the planets. As the visiting star got close to the sun it would have raised on opposite sides of the sun two tidal waves which would have increased to an enormous height as the distance between the two bodies diminished. Finally the two waves would have leapt up from the sun's surface in the form of two gigantic jets of gas. During this process the visiting star would have been swinging rapidly round the sun, and so would have imparted to the sun a rotation on its axis in the same direction. But if the two jets of gas were ejected by a rotating body they would have emerged not in straight lines but in the form of two spiral arms. The authors believed that this was the stage represented by the spiral nebulae. In the next stage, it was supposed, the jets would break up into small masses of material which would gradually collect together by conglomeration to form larger masses; and these, it was thought, were represented by the condensations observed in the arms of spiral nebulae. The large masses would gradually have cooled down to form the planets; while the rest of the ejected material would partly have fallen back on the sun and partly have been dispersed throughout space.

An obvious criticism of this theory is that an approach of two stars close enough to bring about the events described must be an excessively rare occurrence. But while it seems possible that an excessively rare, though not necessarily unique, occurrence may explain the origin of the solar system, it is impossible to suppose that it also explains the formation of the spiral nebulae which are literally to be counted in millions. A further point is that, as we now know, the spiral nebulae are to be compared in size not with the solar system but with the galaxy.

The most important contributions during recent times to the problems under discussion are the investigations of Sir James Jeans which he first embarked upon over thirty years ago. A detailed account of the earlier part of his work was given by him in his Adams Prize Essay for 1917; while in 1928 he published his *Astronomy and Cosmogony* which embodied his later investigations as well. We shall now describe briefly the theories he has put forward as the result of those investigations which bear upon the formation and evolution of (1) galaxies, (2) star-clusters, (3) double-stars, and (4) the solar system.

Galaxies. If all the matter in our own galaxy and in the extra-galactic nebulae was distributed in the form of gas evenly throughout the intervening spaces, it would be in an extremely rarefied condition. The density it would have can be roughly calculated, and the figure obtained is in satisfactory agreement with that found by Sir Arthur Eddington on the relativity theory of the Expanding Universe. The random movements of the gas molecules in this primaeval and undifferentiated universe would bring about from time to time localized accumulations in which the molecules would be somewhat closer together. One of two things would then happen: either the accumulation would disperse again as a result of gas pressure—just as the air let out of a bicycle tyre disperses into the atmosphere; or the accumulation would grow larger as a result of its gravitational attraction dragging into it more gas from the outside. The point that settles which of these two opposing tendencies will overrule the other is the *weight* of the accumulation. Sir James Jeans has shown that owing to the excessively low density of the gases in this primaeval universal nebula no accumulation could have remained permanent unless it had many million times the weight of the sun. Now since the galaxies have weights of this order, he has made the highly interesting suggestion that the accumulations in the rarefied universal nebula were in fact the extra-galactic nebulae: that the formation of the extra-galactic nebulae was the first step in the differentiation of the primaeval chaos.

The gaseous accumulations if left to themselves would slowly contract and form non-rotating spherical bodies. But since there will be other accumulations moving about in their neighbourhood, these will exert tidal actions and induce a slow rotation. Sir James Jeans has shown that a spherical mass of gas which is rotating will, as its contraction proceeds and its rotation gets faster, first become increasingly flattened at its poles and then become lens-shaped with a sharp edge to its equator. Any further speeding up of the rotation will cause the gaseous material to flow outwards around this sharp edge, forming a disc-like rim to the lens-shaped mass. But in order that the material may flow out evenly from *all* points round the edge, either the mass must be alone in empty space or the gravitational attractions of other masses acting upon it must be precisely equal in all directions. Sir James Jeans pointed out that even a minute excess of the gravitational attraction in any one direction will upset the balance, and cause the ejection of the material to take place from *two points* on the edge diametrically opposite one another, instead of from all points simultaneously. The ejection once started from these two points by the minute asymmetry in the surrounding gravitational field will proceed automatically, for the asymmetry in these two directions will now be increased by the added gravitational attraction of the material already ejected. The two long gaseous arms will grow continuously at the expense of the central lens-shaped mass as the ejection proceeds. And finally the arms will become unstable and be broken up.

It will be seen at once that the developmental stages of the original gaseous accumulations as deduced theoretically by Sir James Jeans correspond almost exactly to the various types of extra-galactic nebulae: the globular nebulae, the bun-shaped or " elliptical " nebulae, the lens-shaped nebulae, and the spiral nebulae in which the arms gradually increase at the expense of the central lens-shaped nucleus and finally break up into condensations. Further, there are one or two nebulae known which, instead of possessing two antipodal arms, are surrounded at their periphery by a disc-like ring or a series of rings. This rare form of nebula would correspond to the gaseous accumulation in the theory which happens to be so placed in space that the gravitational attractions exerted on it by the surrounding accumulations are the same in all directions: the ejection then takes place evenly all round the sharp edge exactly as was deduced by Laplace in his Nebular Hypothesis.

The theory, however, fails to explain the spiral form of the arms of the extra-galactic nebulae, and why it is that in practically all spirals the arms show approximately two convolutions about the centre and no more. If the material was continuously in process of travelling outwards along the arms, as the theory suggests, the arms ought to be continually increasing in length and the convolutions in number. We should then find younger spirals with few convolutions and older spirals with many; but we do not. Undoubtedly the spiral with two convolutions is the form permanently retained by some means or other during the development of the extra-galactic nebulae in their later stages. On the other hand we know definitely from observation that the spiral nebulae are rotating about their centres; and Dr. Slipher of the Lowell Observatory has shown that the direction of their rotation is invariably the same as that of a spring being wound up. So far it has been found impossible to give any physical theory that will reconcile this permanence of the spiral configuration with the rotation of the nebulae. As Sir James Jeans has himself pointed out, this failure must necessarily weaken one's confidence in the other aspects of the theory of the development of extra-galactic nebulae. Writing in 1928 he said, " Each failure to explain the spiral arms makes it more and more difficult to resist a suspicion that the spiral nebulae are the seat of types of forces entirely unknown to us. . . . The type of conjecture which presents itself, somewhat insistently, is that the centres of the nebulae are of the nature of ' singular points ' at which matter is poured into our universe from some other, and entirely extraneous, spatial dimension, so that, to a denizen of our universe, they appear as points at which matter is being continually created." Other eminent mathematicians, including Professor E. W. Brown of Yale, who is the greatest living authority on the motion of the moon, have worked on this problem; but there is still no satisfactory explanation of the spiral arms.

There is another weak point in the theory. At the time that it was being worked out, the exact nature of the extra-galactic nebulae was still very much in the air. It was then reasonable to suppose that the unresolved globular and elliptical nebulae and the unresolved nuclei of the spiral nebulae were in the gaseous state. The most recent investigations, as we have seen, suggest strongly that all the extra-galactic nebulae are systems of stars, which however may be too faint to be visible individually. It is true that some of them contain gaseous nebulae within them, and that some of the globular and elliptical nebulae may be largely composed of undifferentiated gas or of fine dust particles. Even so, however, these systems must be *essentially* stellar systems; for it is difficult to see how a mass of gas or dust in which few or no stars were enveloped could possibly have the extremely high luminosity of the extra-galactic nebulae. The spectroscopic evidence also points to the same conclusion. Now the theory of Sir James Jeans is essentially a theory of the evolution of rotating masses of *gas*. As Professor Russell has pointed out, although it is possible, if not probable, that an immense rotating swarm of stars, such as we believe the extra-galactic nebulae to be, might behave in a similar way to a mass of gas of the same dimensions, this has not yet been definitely proved. In that case it would be necessary to suppose that the stars had already to a large extent crystallized out of the original gaseous accumulations by the time these accumulations, through contraction and increasing speed of rotation, had arrived at the globular stage. In the still earlier stages the accumulations, which would then be of less regular form and as yet undifferentiated into stars, would be practically non-luminous and thus invisible. We conclude that although there is a remarkably close agreement between theory and observation there are certain serious difficulties to be overcome before we can regard our ideas on the evolution of the extra-galactic nebulae as in any sense final.

From the purely astronomical point of view the most interesting part of the foregoing theory is probably that in which Sir James Jeans discusses the *origin* of the extra-galactic nebulae, the development of the gaseous accumulations in the primaeval chaos. This part of the theory moreover does not seem to be confronted with any serious difficulties. It gives good reason for supposing that the extra-galactic nebulae were formed directly out of the much more rarefied gas which was originally diffused evenly throughout space; it explains the manner of that formation; it explains why the extra-galactic nebulae have all very much the same weight; and finally it explains why they have the particular weight which they are found to have. But the same ingenious argument was able to carry Sir James Jeans still further. He was able to show that when the original gaseous accumulations had contracted down to approximately the size that the extra-galactic nebulae now have, much less heavy secondary accumulations would form and be able to remain permanent within the original ones. The limiting weight which determines whether an accumulation is to disperse or remain permanent depends mainly on the density of the gas in which the accumulation appears. In the primaeval gas, with its excessively low density, the limiting weight was approximately the weight of a whole galaxy. But when one of the original accumulations has contracted to the size of the present galaxies, so that its density though still incredibly low is enormously greater than before, the limiting weight for the formation of secondary accumulations within it will be roughly of the order of that of the average star. So this part of the theory has gained for itself still stronger support, by giving a satisfactory explanation how the stars came to condense out of the galaxies, why the stars are all very much of the same weight, and why finally they have the particular weight which they are found to have.

Star Clusters. As to the manner in which star clusters are originally formed we know nothing. It may be that sometimes large numbers of secondary accumulations just happen to form close together in a galaxy; in which case the resulting cluster of primitive stars would retain its compactness partly because the individual stars would probably all be moving with the same velocities, and partly because of their mutual gravitational attractions. But almost certainly there is much more in it than this: for otherwise how shall we explain why the clusters, especially the globular clusters, are built so nearly to a particular scale?

A theory as to the possible relation between open clusters and globular clusters has however been put forward by Sir James Jeans. He has shown that a cluster of stars left to itself would naturally assume the spherical form characteristic of globular clusters. But if such a cluster was frequently disturbed by the gravitational attractions of passing stars the spherical form would gradually be lost. He has suggested that the reason that globular clusters are found only at great distances from the plane of the Milky Way where stars are scarce, while open clusters always occur close to that plane where stars are numerous, is that any globular cluster which approaches the Milky Way is bound to be distorted into an open cluster by passing stars.

Double Stars. The question as to what changes occur in the shape of a liquid body as it rotates more and more rapidly has been the subject of numerous investigations by many of the most celebrated mathematicians of the past. Foremost among these were George Darwin and Henri Poincaré. In recent years the studies in which they were the pioneers have been greatly extended by Sir James Jeans. It has been found that the changes in the shape of a rotating mass of liquid are essentially different from those of a rotating mass of gas. A mass of gas which starts as a sphere will, as its rate of rotation increases, first become more and more flattened at its poles, then become lens-shaped with a sharp equatorial edge, and finally eject from two points on this edge jets of gas. A mass of liquid which starts as a sphere and is made to rotate faster and faster will also become increasingly flattened at its poles. But it will never develop, like the mass of gas, a sharp equatorial edge. Instead the equator, which up to now has been circular, begins to become elongated; and as this elongation increases the body assumes the shape of a rather short and fat cigar. When the length of the cigar has become equal to about three times its greatest thickness, a waist begins to develop near the middle of the cigar and grows more and more pronounced Finally, the waist becomes so narrow that it gives way, and the original body is divided into two. This difference in the behaviour of the gas and the liquid is due to the gas being compressible and the liquid incompressible.

If the compression of a gas is pushed beyond a certain critical point, the gas will pass into the liquid state and become incompressible. There is no doubt that the secondary accumulations or primitive stars, when they first began to condense out of the galaxies, were entirely in the gaseous state. But sooner or later as these stars contracted in size and the gases at their centres became more and more compressed, the central cores must have gradually assumed the incompressible liquid state. Sir James Jeans therefore investigated the more complicated problem of a rotating body which was partly gaseous and partly liquid. He found that if the central parts of a gaseous body were condensed beyond a certain critical degree, it would behave as though it were a rotating mass of liquid, otherwise it would continue to behave as a mass of gas. Since, as we saw, the rotation of a star must continually increase in speed as the star contracts in order to conserve its angular momentum, it follows that if a star has reached the critical degree of central condensation referred to, it will be liable ultimately to divide into two. Whether it does or does not will depend on the amount of rotation with which the primitive star was originally endowed. If the original rotation was slow, it might never speed up sufficiently through contraction to bring about the break-up of the star.[1]

If we suppose a star to have divided in this way into a double star, the two component stars will proceed to separate from one another, as a result of tidal action, just as in the case of the earth and moon. But there is this difference: whereas the earth has over eighty times the weight of the moon, the heavier component of a double star has seldom more than two or three times the weight of the lighter component. It can be shown that because of this difference the distance separating the centres of the two stars, when they are revolving almost with their surfaces in contact, can only at the most be doubled by tidal action. Thus, while this theory may be regarded as giving the most probable explanation of the formation of the large numbers of spectroscopic and eclipsing double stars in which the two components are very close to one another, it cannot possibly explain the formation of the numerous visual double stars in which the components are separated by great distances and take many years to go round one another.

A possible and quite reasonable explanation of the formation of visual double stars with widely separated components is that they were double from the start: two secondary accumulations or primitive stars just happened to arise close together in the galaxy. This would explain the large variations found in the distances separating the components of these double stars and in the elongations of their orbits, and also the occasional occurrence of multiple stars with three or more components.

The Solar System. It might have been thought that Laplace's Nebular Hypothesis, or some slight modification of it, would after all have been found to account for the origin of the solar system. For we saw that Sir James Jeans had shown that a process very similar to that pictured by Laplace would take place in a rotating mass of gas of which the weight was comparable to that of a whole galaxy. But the same author went on to show that what could take place on a galactic scale would not be possible on the scale of the solar system. All would go well with Laplace's original nebula up to the point at which matter began to flow out from its periphery. But this matter, instead of forming jets or rings which could break-up into planets, would possess so little weight and so slight a tendency to hold together under its own gravitational attraction, that it would simply dissipate itself away slowly into space.

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[1] Recently it has become possible to make direct determinations of stellar rotations. The method depends on the broadening of the spectral lines, since one side of the star is approaching us and the other side receding from us as it rotates. But it is not sufficient to measure the breadth of a line : one must determine the exact way in which the intensity of the line falls off on either side of its centre. The method was first developed by Drs. Shajn, Struve and Elvey about 1930, and has since been extended by Professor Carroll. With further improvements the data obtained should be of interest in connexion with the formation of double stars.

After an exhaustive consideration of the problem Sir James Jeans has concluded that the only possible way of attempting to explain the origin of the solar system is by assuming, as did Chamberlin and Moulton, that long ago some other star approached very close to the sun. He pointed out that the approach would have to have been a very close one, with the result that the tidal wave raised upon the side of the sun nearer to the visiting star would have been much larger than the one on the opposite side. The larger wave would have risen higher and higher until it was actually sucked out of the sun as an enormous cigar-shaped filament. The filament being unstable would at once have broken up into discrete portions which later contracted down to form the planets. Since the filament was broader in the middle than at its two extremities, the more massive planets would have been formed in the middle and the less massive planets at the extremities—which fits in satisfactorily with the actual arrangement in the solar system: Mercury, Venus, the Earth, and Mars which are the smallest planets and the nearest to the sun; then the most massive planets, Jupiter and Saturn; and finally smaller planets again, Uranus, Neptune and Pluto, which are furthest from the sun. It will at once be asked how it is that the filament drawn out by the passing star is able to condense into planets, while the material flowing out from the periphery of Laplace's nebula dissipates itself away into space. In the second case the gas flows out comparatively slowly as the nebula contracts and diffuses away immediately. It is unable to hold together under its own gravitational attraction, because there is never enough of it in a small space at one time. The tidal formation of the filament on the other hand is catastrophic: a large mass of material is drawn out of the sun, so to speak, in a single lump. Volume for volume its weight is much greater than that of the material which flows out of Laplace's nebula. Consequently it is able to hold together under the influence of its own gravitation.

The theory of Sir James Jeans also gives a possible explanation of the formation of the satellite systems. With the exception of the earth-moon system, the satellite-systems are remarkably similar in miniature to the solar system: for just as the planets are very small in comparison with the sun so are the satellites very small in comparison with the planets. This similarity suggests that the two types of system were formed as a result of similar processes. Because of the disturbing action of the visiting star, the planets immediately after their birth might have been moving in highly elliptical orbits which would carry them periodically very close to the sun. In that case the sun by its tidal action would tear out of the planets filaments similar to, though much smaller than, that which had been torn out of itself; and these, as Sir James Jeans has shown, would likewise break up into discrete portions and form the satellites. After this no further repetition of the process could occur; for the satellites being so small would have solidified immediately on being born and so would not be in a condition to propagate a third generation. In fact the theory suggests that it was for this same reason that some of the smaller planets did not produce satellites. Thus the smallest planet Mercury and the third smallest planet Venus are without satellites.

One of the most interesting points about this theory of the origin of the solar system is that it depends on the very close approach of two stars; and this must be an excessively rare occurrence. The rarity of the occurrence can be roughly calculated from a knowledge of the closeness with which the stars are packed in space. Adopting the longer " time-scale "—about five million, million years as the average age of the stars—Sir James Jeans finds that about one star in every hundred thousand would be expected to be surrounded by a family of planets. If we regard the shorter " time-scale " as giving a more probable estimate of the average age of the stars—about fifty thousand million years, or one hundred times shorter than the longer " time-scale "—the probable frequency of planetary systems among the stars reduces to one star in every ten million. Such a conclusion is in striking contrast to the ideas frequently expressed thirty or forty years ago. At that time there was of course very little to go upon, and perhaps for that very reason rash speculation was rife. Writers, even of high reputation, were apt to regard almost every star as a possible centre of a planetary system: it seemed quite likely that the universe was fairly teeming with life.

In conclusion it may be said that though the extensive investigations of the last thirty years into the formation and development of the various types of celestial systems have told us nothing for certain, they have greatly contributed to astronomical knowledge.

They have exposed what was fallacious or purely speculative and cleared it away; they have revealed the extreme complexity of much that was thought to be simple, and thus opened our eyes to fresh problems; and finally they have given us in certain cases explanations which probably approximate to the truth.

CHAPTER XXIV

RECENT WORK IN SOLAR PHYSICS

In this final section of the book we shall turn back again to the solar system and see what advances have been made in our knowledge of those bodies which are our nearest neighbours in space. We shall deal in turn with the sun, with the various planets and their satellites, and last of all with the comets and meteors.

The sun, as we have seen, is an ordinary star rather below the average in size and brightness. It is a dwarf star of spectral class G, so that its surface is presumably cooling as further contraction proceeds. Though the sun is but an ordinary star, it is enormously larger and heavier than any of the planets which revolve round it. It weighs about 330 thousand times as much as the earth, and over a thousand times as much as Jupiter the largest of the planets. It is a sphere of gas some 860,000 miles in diameter, intensely hot and luminous. In spite of this the surface of the sun seen in the telescope is outlined with perfect sharpness. The reason is that the opacity of the gas forming the surface increases very rapidly with the depth and thus prevents us from seeing *into* the surface more than a short distance. It is on this surface that sun-spots make their appearance looking almost black by contrast with it. Above the visible surface come the much more rarefied layers of the solar atmosphere and the excessively tenuous corona, none of which can be seen when the sun is examined in the ordinary way with the telescope. The atmospheric layers, which constitute the chromosphere, consist of incandescent gases at a somewhat lower temperature than those which form the surface. These cooler gases absorb part of the light coming from below them and thus give rise to the dark lines in the solar spectrum which make their identification possible. The majority of these gases are confined to a shallow layer called the " reversing layer " a few hundred miles deep lying at the bottom of the chromosphere; but some of the gases, particularly hydrogen and calcium, rise to greater heights and form the upper parts of the chromosphere. Often these high lying gases are projected as prominences to enormous distances far above the general chromospheric level. These various atmospheric layers are ordinarily visible only with the aid of the spectroscope, but during total eclipses of the sun they can be seen directly with a telescope or even with the naked eye.

Outside everything else lies the corona, whose curving streamers may extend to several million miles above the sun's surface. Until very recently it was impossible even to detect the presence of the corona except during a total eclipse.

The study of the sun is important for its own sake. But since the sun is just an ordinary star and can be studied in infinitely greater detail than any other star in the universe, its investigation is invaluable in teaching us about the nature of stars in general.

We have discussed already the early work at eclipses on the chromosphere, prominences and corona, and the discovery of the method of viewing the chromosphere and prominences in ordinary daylight with a spectroscope; the various determinations of the sun's distance; the discovery of the sun-spot period with its attendant phenomena, of the connexion of sun-spots with magnetic storms, and of the fact that the sun does not rotate as a solid body; and finally the various investigations, up to about 1913, into the nature of sun-spots, faculae and prominences.

During the last twenty or thirty years the advance in solar research has been rapid. But since a large part of the recent work in solar physics has been intimately bound up with astrophysics in general, and since this part of it has already been considered in the sections of the book dealing with the stars, we shall now discuss only that part which has a unique application to the sun.

The best determination of the sun's distance is still that which was published in 1910 by Mr. Hinks as a result of his investigation of the observations of the minor planet Eros during its close approach to the earth in 1900. In that year Eros came within thirty million miles of the earth, but in 1931 it came nearly twice as close and was only about sixteen million miles away. Never before had such favourable conditions been available for measuring the sun's distance. An extensive international campaign was organized in which observatories in all parts of the world took part. Photographs were taken of Eros for the measurement of its position among the stars on every available night during the months in which it was nearest. Though six years have now elapsed the final result is not yet available, for the amount of work entailed is enormous. When everything is finished the accuracy with which the sun's distance is known will almost certainly be increased. The figure, however, will probably be very close to that already adopted, and lie somewhere between 92,800,000 and 92,900,000 miles. People are apt to wonder why so much labour should be expended on a comparatively slight improvement in our knowledge of the sun's distance. The reason, as we have said, is that on this measurement all other astronomical measurements depend, whether it be in the solar system, the galactic system or the metagalactic system.

In solar physics as in other branches of astronomy the erection of new and more powerful instruments and the invention of new kinds of instruments have played an important part. Several new " tower telescopes " similar to the two original ones at Mount Wilson have now been constructed. The first of these was put up at the Arcetri Solar Observatory in Italy under the directorship of Professor Abetti. Another, known until recently as the " Einstein Tower," was erected at Potsdam soon after the war. In England there is one at the Oxford University Observatory which was built in 1935 to the design of Professor H. H. Plaskett, the director of the observatory. Of particular interest is the tower telescope of the Mc.Math-Hulbert private observatory in Michigan which is being used in the cinematography of the prominences.

A new instrument based on the principle of the spectroheliograph and known as the spectrohelioscope has during the last few years been installed at many observatories. It was invented by Professor Hale of Mount Wilson and first described by him in detail in 1924. It accomplishes for the benefit of the eye exactly what the spectroheliograph accomplishes for the photographic plate. In the latter instrument an image of the sun is focused by means of a lens on the narrow slit of an ordinary spectroscope. The light after passing through the slit is formed by the spectroscope into a spectrum, which is focused on a second narrow slit. Behind the second slit, and almost in contact with it, is a photographic plate. The position of the second slit is adjusted so as to isolate from the spectral band any required spectral line, in order that only the light from that particular line shall fall on the plate—for the lines in the solar spectrum are not really dark, they only appear dark by contrast with the rest of the spectrum. If a simple exposure were now made we should obtain in the light of the particular line isolated, say one of the hydrogen lines, a picture of that very narrow strip of the sun's image which is lying on the first slit. But what we require is a photograph of the whole sun; and in order to achieve this the sun's image is made to travel slowly over the first slit, while the photographic plate is made to travel in unison with it over the second slit. Thus by a continuous exposure one builds up from a succession of strips a complete picture of the sun's surface in the light emitted by one particular gas. Good pictures clearly depend on the sun's image and the plate moving together in perfect unison and with perfect smoothness, and this is accomplished by means of very delicately controlled motors. Alternatively, instead of moving the sun's image and the photographic plate, one can keep these two fixed and make the two slits move in unison—one across the sun's image and the other across the plate.

The spectrohelioscope adopts the second method and substitutes for the photographic plate an ordinary eyepiece. Moreover, instead of the two slits moving slowly and continuously in one direction until the whole of the sun's image has been included, they are made to oscillate rapidly to and fro and to include only a portion of the sun's image. In this way the eye receives a series of impressions, which quickly succeed one another and rapidly repeat themselves, of a number of contiguous narrow strips of the solar surface. And since owing to the " persistence of vision " the momentary impression of each strip, instead of immediately fading, remains until the next oscillation of the slits renews it, the whole series of impressions becomes fused into a continuous picture of a portion of the sun's surface. Observations with the spectrohelioscope are practically limited to the light in the hydrogen line in the red end of the spectrum, for the calcium lines and the other hydrogen lines are situated in the blue and violet parts of the spectrum where the eye is much less sensitive. But the spectrohelioscope has the advantage over the spectroheliograph as far as the sun's hydrogen atmosphere is concerned, because with it one can watch the progress of the rapid changes going on in the clouds of hydrogen. Particularly in the hands of Professor Hale, and of Mr. Newton at Greenwich, this instrument has yielded valuable information as to the movements and behaviour of these clouds and their relation to sun-spots. In 1935 the International Astronomical Union arranged that a chain of spectrohelioscopes should be distributed more or less evenly round the world in order that the hydrogen atmosphere of the sun should be kept under practically continuous observation.

Probably the most sensational advance in the methods of solar observation has been the achievement of M. Lyot, of the Meudon Observatory in Paris, in detecting the light of the corona and even photographing the corona itself in ordinary daylight. The reason why the corona cannot normally be detected is that its radiations are so feeble that they are completely drowned by the large amount of ordinary sunlight scattered by the dust and moisture in the atmosphere. In the case of the prominences this difficulty is got over by diminishing the intensity of the scattered daylight by spreading it out into a very long spectrum; the prominences, whose light is concentrated mainly in a few isolated lines of hydrogen and calcium, are then readily observed by looking at them or photographing them in the light of one of those lines. But in the case of the corona the light is not concentrated into a few isolated lines; moreover, it is much fainter than that of the prominences. If the earth were deprived of its atmosphere, or if we could observe from the moon which has no atmosphere, the prominences and corona would be visible at any time even to the naked eye. We should simply have to hold at arm's length some small round object which was just large enough to cover the actual disc of the sun and shield our eyes from its direct light; the corona and prominences would then stand out clearly against the perfectly black background of the sky. We can approximate to these conditions by going up on to a high mountain where the sky, owing to the more rarefied atmosphere, is a much darker blue, or much more nearly black, than it is at ordinary altitudes.

For the last fifty years astronomers have been attempting to photograph the corona from the tops of high mountains; but all attempts were unsuccessful until M. Lyot started his observations from the summit of the Pic du Midi in the summer of 1930. There was nothing essentially new in M. Lyot's methods, his success was due rather to his extremely skilful technique in getting rid of the least vestige of dust from the optical parts of his apparatus; because particles of dust on the lenses or prisms will scatter the sunlight in just the same way as does dust in the atmosphere. The observations were conducted at a height of about 8,500 feet. The telescope was small and of quite simple and ordinary design except that there was placed just in front of the eyepiece, where the image of the sun was focused, a round disc just large enough to cover and hide from view the disc of the sun. Looking directly through this telescope in the ordinary way he was able to see the rose-coloured prominences projecting from the sun's edge—the first occasion that a prominence had been seen outside of a total eclipse without the assistance of a spectroscope. By removing the eyepiece and replacing it by a spectroscope, the slit of which was placed close to, and tangential to, the sun's hidden edge, he was able to make out, superimposed upon the ordinary spectrum of sunlight, the red and green lines of the coronal spectrum. He also secured photographs of these lines; and, both from the visual and photographic observations, got measurements of their positions in the spectrum which compare in accuracy with the best measurements obtained during total eclipses. This was the first time that the coronal spectrum had been observed, by any method whatsoever, except during a total eclipse. Finally, M. Lyot attempted to detect the direct light of the corona by means of a delicate polariscope. We have already alluded to the polarization of light when discussing the polarization methods for comparing the brightness of the stars (page 90). Here it need only be said that when a substance polarizes the light which it reflects, the brightness of that object when viewed through a Nicol prism will vary as we rotate the prism and alter the orientation of its axis. If polarization is complete then a particular orientation of the prism will cut out all the light reflected from the object, and it will look black. As the prism is rotated the light will return until, when the prism has been rotated through a right angle, the brightness of the object reaches a maximum; a second rotation through a right angle again obliterates the object, while a third one once more brings it to maximum brightness. Usually polarization is only partial, so that as the prism is rotated the object fades to a minimum brightness but is not completely darkened. Eclipse observations had already shown that the light of the corona was partially polarized. By employing the polariscope in conjunction with his special corona-telescope, M. Lyot was able to show that the degree of polarization of the light close up to the sun's edge differed at different points round the edge. Presumably the points at which the polarization was greatest were the points at which the light of the corona was most intense. So although he was unable to see or photograph the actual form of the corona, he was able roughly to map out its form from his polariscopic measurements.

In the following year, 1931, M. Lyot had still greater success at his mountain station. He found that with his simple camera (the telescope just described) he was able, with panchromatic plates and red filters, to obtain on days of the best transparency direct pictures of the corona. At first it was thought that these great achievements might revolutionize our study of the corona and do away with the necessity of expensive and time-consuming eclipse expeditions. But this is improbable; for the new methods are confined strictly to the lowest and brightest layers of the corona; and, even there, they can tackle only the brighter structural details of the corona and the more prominent lines in the coronal spectrum. Their great advantage is that they can be used on any day in the year when the sky is clear enough; and thus, while limited in scope, they should be able to supplement in many important respects the eclipse observations which can be made only during a few minutes every year or so.

The daily photography of the sun's surface as a record of its degree of spottedness was first urged upon astronomers by the younger Herschel and was commenced at Kew in 1858. The Kew records ran till 1872; the work was then transferred to Greenwich, and from 1874 to the present day the Greenwich record is continuous. By arrangement with the Cape Observatory and the solar observatory at Kodaikanal in India, where the same routine is carried out, most of the gaps which occur in the Greenwich record owing to clouds are filled in. The Greenwich method is to record for each day the positions of the individual spots and the fraction of the sun's surface which is covered by spots. An alternative method for expressing the sun's spottedness was suggested by R. Wolf of Berne, who in 1853 independently discovered the connexion between magnetic storms and the cycle of sun-spot activity. The spottedness of the sun at a given time was expressed by a certain number which depended on the number of spot groups visible, the total number of spots counted, and the size of the telescope employed. The method has the advantage that the observations are made very simply and rapidly, and further, since the size of the telescope is taken into account, observations made with a small telescope can be directly compared with those made with a large telescope. In this way Wolf was able to embody in a uniform scheme not only his own observations and those of other observatories, but also many of the observations of the earlier astronomers. The work first started by Wolf has now developed into an international organization which is carried on by Drs. Wolfer and Brunner of Zürich. Each year they publish the "Wolf sun-spot numbers" for each month as derived from observatories all over the world; and in 1925 Dr. Wolfer was able to give a complete list of the monthly spot numbers from 1749 to 1924, a period of 175 years.

The Greenwich observations and the results obtained by Wolf and his successors have naturally led to a much more detailed acquaintance with the sun-spot cycle. It is found that the period which averages about eleven years is by no means regular; it may sometimes be as short as eight years and sometimes as long as seventeen years. Carrington's discovery, that at the start of a new cycle spots appear in high latitudes and gradually work towards the equator where they die out at the end of the cycle, has been very fully confirmed by Maunder's analysis in 1913 of the Greenwich observations. At the same time Maunder also examined the Greenwich magnetic observations and made a discovery of considerable interest. The relation between the frequency of magnetic storms and the sun-spot cycle had long been known, but the new discovery was concerned with a relation between individual magnetic storms and individual sun-spots. Maunder found that a big magnetic storm generally coincided with the presence of a large sun-spot near the centre of the sun's disc; that a large magnetic storm was liable to be repeated by a second storm after an interval exactly equal to that required for the spot to return again to the centre of the disc—which owing to the earth's motion is 27·3 days and not 25; and finally that, if a storm is associated with a spot, it commences about thirty hours after the spot has passed nearest to the centre of the disc. But all big spots do not produce storms, and all big storms are not associated with spots. Presumably therefore the storms are due to phenomena occurring on the sun which though usually connected with spots are not necessarily connected with them. It is now generally believed that magnetic storms are due to electrically charged particles, which are ejected from these disturbed portions of the surface in a fairly narrow beam, and which if they hit the earth's atmosphere produce electrical and magnetic phenomena. It is interesting that the 30-hour interval gives a velocity for these particles in agreement with recent theoretical investigations by Professor Milne.

Carrington's early work on the rotation of the sun and his discovery that its period of rotation grew gradually longer as one passed from its equator into higher latitudes, are also confirmed by modern observations; in fact the figures for the rotation periods in various latitudes derived from the Greenwich observations by Maunder in 1924 differ very little from Carrington's original figures. The spots, however, are found not to be stationary relative to the sun's surface. According to Maunder the rate and direction of their drift is dependent on their age. This drift naturally complicated the problem of determining the rotation periods of the sun from spot observations; and to overcome the difficulty it was necessary to analyse the observations of large numbers of spots so that the irregularities could be taken into account.

Since the discovery in 1868 of the method for observing prominences in daylight, the daily recording of their numbers and positions has become a routine at many observatories. The longest series of visual observations of this kind is that which was started by Respighi in Rome in 1869 and which was published without interruption until 1911. Since 1922 a similar record has been published regularly by the Arcetri Observatory. For many years the Kodaikanal Observatory in India has published a six-monthly record derived from its own photographic observations of prominences and from those of other observatories. Observations of prominences over a long period have been made by Mr. Evershed at Kenley and Kodaikanal between 1890 and 1923 and in latter years at his private solar observatory at Ewhurst in Surrey. From such observations it is found that the prominences follow the spot cycle, being most numerous at spot maximum and least numerous at spot minimum. The prominences occur mainly in four zones of solar latitude. Two of these are almost identical with the two spot zones and lie just north and south of the equator. The other two zones are much nearer the sun's poles, and lie roughly between fifty and seventy degrees north and south latitude. The prominences in the two spot zones follow closely the activity of the spots and like them tend to converge on the equator as the cycle approaches a minimum. But the prominences in the high latitude zones appear first, soon after spot maximum, in comparatively low latitudes, and then gradually approach the poles where they die out just before the next maximum; at spot minimum their numbers are also diminished. The prominences that occur in the spot zones are occasionally in close association with the spots, rising out of the chromosphere immediately above them. The great majority of prominences, however, do not show this association.

As we have seen, prominences are of two types: quiescent and eruptive. The quiescent prominences may reach enormous dimensions, they change in shape comparatively slowly and remain suspended above the chromosphere for a long time, sometimes several months. They are practically never connected with spots and consist mainly of hydrogen and calcium vapour. The eruptive prominences, which are usually small, are short-lived and change rapidly in form. They are frequently connected with spots, generally young spots, and often consist of numerous metallic vapours in addition to hydrogen and calcium. Clearly then such prominences must involve the chromosphere down to its deepest layers, just as one would expect if they originate from the spots.

We have already referred to the earlier work in photographing the sun's atmosphere in the light of hydrogen or calcium vapour by means of the spectroheliograph. And we saw that if we set the second slit of this instrument on the *centre* of the calcium line, for example, we got a picture of the high level calcium clouds, while if we set it on the *edge* of the line we got a picture of the low level calcium clouds. The reason for this is that the vapour at a low level is denser than the same vapour at a higher level, and a dense vapour gives rise to a broader spectral line than a rarefied vapour: hence the outer edge of a spectral line comes entirely from the densest and lowest level gases, while its centre comes mainly from the rarefied high level gases. Photographs of this kind had already shown that the bright specks or faculae which we see directly with the telescope near the sun's edge, often in connexion with a spot, are due to low level calcium clouds. The high level calcium clouds, which are not seen directly with the telescope, are more extensive and often almost obscure the underlying spots. We also saw that the photographs taken in hydrogen light showed both bright and dark hydrogen clouds, and that the dark clouds were prominences seen in silhouette against the sun's disc. Unlike the calcium clouds those of hydrogen often show a cyclonic or vortical arrangement around a spot.

Owing to the increased number of spectroheliographs now in use and to the invention of the new spectrohelioscope, an immense amount of information has lately been accumulating as to the distribution and movements of the gases in the sun's atmosphere. This detailed information is, however, very complex; and many years of more or less continuous study of the changes taking place in the sun's atmosphere will be necessary before we shall be able to draw from it much in the way of general conclusions. We shall, therefore, mention here only a few of the recent results obtained.

It now appears that the dark hydrogen clouds, that were previously recognized as prominences seen in silhouette, are generally prominences of the quiescent type, which are long lived and usually lie at high levels above the chromosphere. It seems on the other hand that certain of the most brilliant of the bright hydrogen clouds are prominences of the eruptive type; for these brilliant hydrogen clouds, or hydrogen eruptions, develop very suddenly and undergo rapid changes like the eruptive prominences. The identity of these two types of clouds with the two types of prominences has sometimes been well shown by spectroheliograms in which these clouds are seen near the sun's edge and at the same time projecting beyond it. Some observers believe that these brilliant hydrogen eruptions revealed by the spectroheliograph and spectrohelioscope are more directly connected with magnetic storms than the sun-spots themselves. Since bright hydrogen eruptions, though most often seen in the immediate neighbourhood of large spots, sometimes occur where there is no spot or only a small spot visible, this view would fit in with the fact that magnetic storms do not always follow the appearance of a large spot and sometimes occur when no spot is visible.

As a result of improved apparatus and technique Dr. d'Azambuja at Meudon Observatory, Paris, has lately been using, in addition to the lines of hydrogen and calcium other lines including those of iron, magnesium, sodium and strontium, and has thus been getting photographs of the sun's atmosphere in the light of these vapours as well. He finds that the clouds of these gases have much the same structure as that of the calcium clouds and never show the cyclonic or vortical structure so often exhibited by the hydrogen clouds.

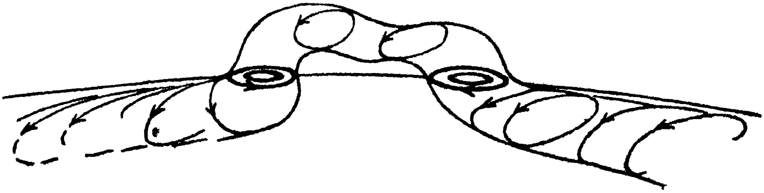
The rules governing the direction in which the hydrogen vortices surrounding sun-spots rotate, now seem to be well established. When large spots occur, as they generally do, in pairs, the two spots lie always roughly east and west of one another and their vortices always rotate in opposite directions. It is found that the vortices surrounding all the western spots in the northern hemisphere rotate in the same direction, but in a direction opposite to that of the vortices surrounding the western spots in the southern hemisphere. Further, when a fresh spot cycle starts, with the appearance of spots in higher latitudes, the directions in which the vortices rotate are reversed in the two hemispheres. Thus during one cycle if the western vortices in the northern hemisphere are rotating clockwise, the western vortices in the southern hemisphere will be rotating anti-clockwise; during the succeeding cycle the western vortices in the northern hemisphere will rotate anti-clockwise, and the western vortices in the southern hemisphere clockwise. In the case of the eastern vortices the directions of rotation and their changes will be exactly the opposite. We have previously mentioned Professor Hale's discovery of a similar set of rules governing the magnetic fields surrounding the spots: if one spot of a pair corresponds to the north pole of a magnet, the other corresponds to its south pole; the arrangement of the polarity is the same for all spot pairs in one hemisphere, and opposite to that of the spot pairs in the other hemisphere; and at the commencement of a new spot cycle the arrangement in the two hemispheres is reversed. This discovery of Professor Hale has been fully confirmed by the work done during the last two cycles; so there can be no doubt that the polarity of the magnetic field round a sun-spot is controlled by the direction of rotation of the hydrogen vortices.

It might be supposed that apart from the intense local magnetic fields surrounding sun-spots, the sun as a whole would also have a magnetic field just as the earth has. A magnetic field is detected spectroscopically by a characteristic doubling or tripling of the spectral lines. This doubling and tripling of the lines is well seen in the spectrum of sun-spots, where the magnetic field is very intense. It was to be expected, however, that the general magnetic field of the sun, being much weaker, would spread apart the double or triple lines so slightly that instead of seeing them double or triple one would see them merely rather wider than normal. With their very powerful spectrographs the Mount Wilson observers claim to have detected and measured this widening of the lines due to the sun's general magnetic field. They find that the field is about one hundred times as intense as that of the earth, and that as on the earth the sun's north magnetic pole is near to, but not exactly at, its north pole of rotation. But the observations are extremely difficult; and though it is highly probable that the sun has a magnetic field—as is so strongly suggested by the structure of the corona in the neighbourhood of the sun's poles—we cannot yet say that it has been detected or measured with complete certainty.

The rotation period of the sun which is normally determined from the successive passage of long-lived spots across the centre of the sun's disc, can also be determined from the various other solar phenomena. In 1924 Maunder determined the rotation periods of the faculae in various latitudes from the Greenwich observations between 1888 and 1923. Determinations of the rotation periods of the calcium clouds shown on spectroheliograms have been made by Professor Hale, and by Kempf at Potsdam. In the case of the bright and dark hydrogen clouds the determination is more difficult because, owing to the rapid changes taking place, one cannot be sure of identifying them over any length of time. Dr. Royds, at Kodaikanal, has however got fairly definite results for the dark hydrogen clouds; but the Mount Wilson results for the bright hydrogen clouds, which are the more difficult to identify, are less certain. Another method for determining the rotation periods of the various solar phenomena is to measure the shifts in their spectral lines on both east and west sides of the sun; for the rotation is carrying the gases on the eastern side towards us and the gases on the western side away from us. This method has been used extensively at Mount Wilson by St. John and Professor Adams, at Kodaikanal by Mr. Evershed and Dr. Royds, at Arcetri by Professor Abetti, at Edinburgh by Mr. Storey and at Pulkowa by Drs. Hase and Peripelkin. In recent years it has yielded exceptionally fine results in the hands of Mr. Evershed at Ewhurst, These various investigations have shown clearly that the higher layers of the sun's atmosphere are rotating more rapidly than the deeper layers. Thus the spots rotate most slowly, then the faculae, then the mid-level calcium clouds, then the hydrogen gases (determined spectroscopically) and lastly, most rapidly of all, the highest calcium gases (also determined spectroscopically). All these layers show like the spots a definite " equatorial acceleration," their rotation periods increasing regularly as one passes from the equator towards the poles; on the other hand the amount of this acceleration is less marked in the higher layers than it is in the lower layers. The only exception to this general rule appears to be the reversing layer whose rotation, determined spectroscopically, is slower even than that of the spots—yet the reversing layer which lies at the base of the chromosphere, must be at a higher level than the spots.

There is good evidence from the spectroscopic observations that the sun's rotation is not quite constant. From 1908 to 1919 the period gradually increased, from 1919 to 1928 it remained unchanged, and since 1928 it has been slowly decreasing again. These variations, which are not shown by the spot observations, are only revealed by the best and most homogeneous of the spectroscopic observations.

A theory which helps to explain some of the more important features of the sun-spot cycle was put forward by the well-known Norwegian meteorologist, Professor Bjerknes, in 1926. We have already seen that a spot is probably a funnel-shaped vortex in which the hot gases from below stream spirally upwards; on reaching the surface they expand and flow outwards over the surface from the mouth of the spot. Professor Bjerknes supposes that during a given cycle all the spots in one hemisphere are part of a *single* tubular vortex which completely encircles the sun just below the visible surface and parallel to the equator. Within this gaseous tube all the gases circulate in the same direction: *not* round and round the sun in the direction of the length of the tube, but round and round its walls as shown in the diagram. From time to time, owing to disturbances deep down in the sun, the



tubular vortex is bent upwards so as to reach and break through the sun's surface. When this happens it will obviously break through at two places close together and produce two vortices opening on to the surface and rotating in opposite directions: again this is made clear by the diagram. It follows that all the westerly spots produced by the vortex will have the same direction of rotation and a direction opposite to that of all the easterly spots, just as we saw was the case for all the spot pairs of a given cycle in one hemisphere. Since the hot gases in the deeper parts of the sun must cool by radiation into space on reaching the surface, a vigorous circulation of the solar gases must be continually in progress. On the earth the direction of this circulation is determined by the equatorial regions being hotter than the polar regions: the hot air rises upwards at the equator, and the cool air rushes over the earth's surface from the poles to take its place. Hence, on the earth the lower current is from the poles to the equator and the upper current is from the equator to the poles. It is difficult to see what determines the corresponding currents in the sun's circulation; but there is reason to suppose that they would be just the other way round. In other words, actually at the surface and just below it, the current would be from the pole to the equator; while at a slightly deeper level below the surface the current would be from the equator to the pole. (To avoid confusion it must be noted that we are here considering not the atmosphere of the sun, but the gaseous layers somewhat below and including its surface.) Now the tubular vortex producing the spots is supposed to lie just below the surface, here and there breaking through it. This vortex will therefore be carried along by the current which is moving towards the equator. It is suggested that this is the explanation of the gradual movement of the spot zone towards the equator as the 11-year cycle proceeds. Now, as we have seen, there must be at a slightly deeper level a current running in the opposite direction, from the equator to the pole. It is supposed that in this deeper layer there is a second tubular vortex, within which it can be shown the gases would be circulating in the direction opposite to that of the gases within the first vortex. Thus, while the high-lying vortex is being carried towards the equator, this second low-lying vortex is being carried away from the equator; and finally, when the first vortex is sucked deep beneath the surface at the equator, the second one rises to the surface in a high latitude. Thus a new spot cycle is started in high latitudes; and the second vortex, which produces spots rotating in the opposite directions to those of the first vortex, now moves gradually equatorwards, while the first vortex lying too deeply to produce spots travels back towards higher latitudes. This theory explains the reversal of rotation at each new cycle, and the drift of the spot zones towards the equator; but it does not explain why the two hemispheres just happen to behave in opposition to one another.

Of the ninety-two chemical elements, only two of which remain to be discovered on the earth, all but thirty-five have been detected in the sun's atmosphere. Some of these missing elements may be found later when their spectra have been more completely analysed and compared with the solar spectrum; others may never be found because the quantities in which they are present may be too small; and others again will certainly never be found even if present, because the lines they give rise to are not in the observable parts of the spectrum. There is absolutely no reason to suppose that any of the elements which go to make up the composition of the earth are missing from the sun. On the other hand there are great numbers of lines in the solar spectrum which are unidentified; but when one realizes that some of the elements, such as iron, are represented by several thousands of lines each in the solar spectrum, this is scarcely surprising. The measurement and identification of the lines is a most laborious matter; and in every case one has to make certain that the line really belongs to the solar spectrum and is not merely produced by absorption in our own atmosphere. The number of lines observed and elements identified is naturally much larger in the solar spectrum than in stellar spectra of the same type. This is simply due to the far greater brightness of the sun which makes it possible to photograph its spectrum on a much bigger scale. So far as we can tell the solar spectrum differs in no way from that of the stars of the same spectral type. It is interesting that while the ordinary solar spectrum is of type G, that of the sun-spots is of type K and that of the chromosphere varies from A to F. The later spectral type of the spots is due to the lower temperature of the gases producing them. On the other hand the earlier types of spectra found in the various levels of the chromosphere, are due to the lower pressures and higher degrees of ionization found at increasing heights above the sun's surface.

In 1927 the sun was totally eclipsed in the British Isles for the first time for 203 years. Many thousands of people took the opportunity of visiting the narrow strip of country between North Wales and Yorkshire from which it was alone visible, but only a small proportion of them were favoured with a clear sky. Of these last there are probably but a few who would not agree that a total eclipse of the sun is among the most magnificent and impressive of all natural phenomena. It is also an event which is of the greatest scientific importance to astronomers.

In Chapter II we have dealt with the numerous discoveries that resulted from the first eclipses which were scientifically observed. Since then, owing to the increasing complexity of the problems studied and of the data obtained, tangible results have been accumulating more slowly. It will be simplest therefore to consider the results from the eclipses of the last sixty years all together. During this period, from 1875 to 1937, there have been eighteen total eclipses; yet the amount of time provided by them for observation adds up to slightly over two hours.

With the great improvements in spectroscopic technique and with the methods devised by M. Lyot, several of the problems that previously could only be tackled at eclipses can now be studied in ordinary daylight. The study of the prominences has been entirely taken over by the observatory, as also to some extent the spectroscopic study of the chromosphere and reversing layer. We saw that at the instant at which the moon just covered the sun's disc there is revealed momentarily a brilliant narrow rim round the sun, whose spectrum is the reverse of the ordinary solar spectrum. Instead of the dark Fraunhofer lines there is a corresponding set of bright coloured lines upon a dark background. At first it was thought that the two spectra, though the reverse of one another, were identical. But soon it was found that the relative intensities of the lines in the so-called " flash spectrum " of the reversing layer differed from those in the ordinary solar spectrum. Still later it was discovered that these differences became more marked as one passed from the reversing layer upwards into the higher layers of the chromosphere. The change with increasing height is in fact a change from an F to an A-type spectrum. As we have seen, the spectral type is determined by the behaviour of the atoms under varying conditions of temperature and pressure; so in this way much has been learnt as to the physical conditions existing in the different chromospheric layers. But though our knowledge of the sun has greatly benefited from atomic theory, as developed from laboratory experiments and the observations of stellar spectra, the benefit has been by no means one-sided. In fact the elaborate studies made of the relative intensities of the lines in the various spectra of the reversing layer and chromosphere have helped to elucidate many puzzling problems in regard to the structure and behaviour of the atom.

Though many very beautiful photographs have been taken of the flash-spectrum in ordinary daylight with the powerful spectrographs of the tower telescopes at Mount Wilson, they often differ markedly from similar photographs taken at eclipses; and undoubtedly they give less reliable results. The photographs at Mount Wilson can only be taken on days when the air is absolutely steady; and even then, during the comparatively long exposures that are necessary, it is impossible to be certain of keeping the slit of the spectrograph at a constant elevation above the sun's surface. The spectrum obtained will thus represent a mixture of the spectra of several different levels. Consequently the best results in this particular problem are still to be obtained at eclipses. At an eclipse only a very few seconds are available for photographing these spectra, because the moon moves rapidly and soon covers up the topmost layers. At most recent eclipses it has become the custom to use a spectrograph with some sort of moving plate or film, so that as the moon moves on and covers, or uncovers, the various chromospheric levels a continuous record is obtained of their successive spectra.

Another piece of information that is to be got from the study of the chromospheric spectrum during an eclipse is the height to which the various gases in the sun's atmosphere rise above its surface. This has been investigated mainly by Professor Mitchell of the University of Virginia. He uses a grating spectrograph without a slit and takes a single short exposure so soon as the sun's disc is exactly covered by the moon. The lines of the various gases appear on these photographs as arcs of circles, the lengths of which depend on how far the corresponding gases protrude beyond the moon's edge. So by measuring the lengths of these arcs he is able to calculate the height to which the different gases rise above the sun's surface. This is the only way in which these heights can be measured directly.

The nature of the sun's corona is still very little understood. Until 1930 not a single aspect of it could be studied except during a total eclipse—which means that the total amount of time during which it has been under observation since the first eclipse expeditions, is considerably less than three hours. Since 1930 it has become possible at specially selected mountain stations and on days of peculiarly great transparency to investigate certain of its features out of an eclipse. A few of the brightest coronal lines can be photographed and measured; and probably in the near future these measures will surpass in accuracy those made at eclipses. The structure of its brightest and innermost layers can be directly photographed and the light from these parts can be investigated with the polariscope. The great advantage of such observations is that presumably it will be possible to carry them on more or less continuously through the year, and thus bring to light the *changes* that must be continually taking place in the long intervals between eclipses.

But the majority of coronal problems are still only to be tackled at total eclipses. The first of these is the study of the general form and structural detail of the corona. For this, it is necessary at each eclipse to take many photographs of different exposures: the short exposures reveal the detail in the brighter inner regions, while the long exposures bring out the fainter outer parts of the long streamers. The general shape of the corona given by the longer exposures has long been known to be closely connected with the sun-spot cycle. At minimum when solar disturbances are rare the corona assumes what is presumably its normal, undisturbed form: long equatorial streamers extend out symmetrically on either side; while short feathery plumes radiate symmetrically from the polar regions, like iron filings near the poles of a magnet. At maximum solar activity streamers sweep outwards in a quite irregular way from all sides of the sun; and though their intensely brilliant bases sometimes coincide with a sun-spot or prominence, it is more probable that their origin is due to disturbances more deeply placed and which do not necessarily manifest themselves in the surface or chromosphere. But there is one almost constant relation between the corona and the prominences; for practically invariably above a prominence the coronal material is arranged in smoothly curved arches. Often there are several arches one above the other and concentric with one another.[1]

There have been numerous attempts to determine the directions and velocities with which the coronal material is moving. One way of doing this is to compare coronal photographs taken from opposite ends of the track of totality. But though the track of totality may extend over many thousands of miles of the earth's surface, the interval elapsing between the times of total eclipse at the two ends is never much more than two and a half hours. At the 1918 eclipse which was visible in America, Professor Miller found that the arches above the prominences were receding from the surface of the sun at the rate of about ten miles a second. This conclusion was confirmed at the 1926 eclipse by Dr. Horn d'Arturo from a comparison of his own photographs taken in East Africa with others taken in Sumatra. So far, however, no evidence has been found of motion in the material composing the great streamers of the corona; we do not even know whether the motion is inwards or outwards. A great deal of work has been done, particularly by Dr. Moore during the expeditions of the Lick Observatory, in trying to detect the rotation of the corona from the shift in its spectral lines on opposite sides of the sun; but no definite success has yet been achieved.

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[1] At the eclipse of June 1937 Major A. W. Stevens, with an American expedition to Peru, obtained aeroplane photographs of the corona from a height of 25,000 feet. These showed, in addition to the ordinary coronal streamers a tenuous envelope of globular form surrounding the sun. Indications of such an envelope had previously been found on photographs by Drs. Bergstrand and von Klüber. But the phenomenon will require further investigation at future eclipses before we can be certain that it is real and not due to scattering of light by our atmosphere or our instruments.

When the corona is examined with a spectroscope it is found that most of the light in all parts of it comes from a continuous spectrum. In the lowermost parts of the corona there is superposed on the continuous spectrum a number of bright lines which cannot be identified with those of any known element; also the relative intensities of these lines vary considerably from one eclipse to another. But in the greater part of the corona, in the middle and outer corona, no bright lines are found; instead there is superposed on the continuous spectrum the ordinary dark lines of the solar spectrum. This suggests that throughout the corona most of the light is due to the light of the sun reflected from small particles, whereas in the inner corona part of the light is actually emitted by atoms. But the question is, what is the nature of these particles and atoms?

Until the most recent eclipses considerable doubt existed as to whether the dark lines in the spectrum really came from the corona or were merely due to sunlight scattered in our own atmosphere. As a result of very elaborate precautions in the technique these doubts have been more or less removed; and it is now practically certain that the middle and outer corona give a dark-line spectrum similar to that of sunlight. This result is confirmed by the polariscopic observations. Here again there was the difficulty in distinguishing between the polarization of the coronal light and the polarization of the light from the sky. But the work, particularly of Turner and of Professor Newall of Cambridge, has shown conclusively that the coronal light is polarized, and is therefore in part at least reflected light.

The particles producing the reflection were at first thought to consist of fine dust and molecules. Dust particles, at least in the inner parts of the corona, would be heated to incandescence, and those nearer the sun would be heated to a higher temperature than those further away. The nearer ones would be white hot and the further ones only red hot; so in that case we should expect the outer parts of the corona to be redder than the inner parts. If the scattering of the light was due to molecules the blue light would be scattered more than the red light, and the corona would be blue like the sky and definitely bluer than the direct light of the sun. Determinations of the colour of the corona, from photometric measurements of its continuous spectrum and from direct photographs taken with colour filters, show pretty definitely that it does not differ appreciably from the colour of direct sunlight, and that there is no increased reddening in its outer parts. It seems probable therefore that most of the reflected light in the corona is due to scattering of sunlight by free electrons, which, it has been shown, scatter light of all colours equally.

The study of the bright coronal lines forms probably the major problem at most eclipses. Even the brightest of them are not very intense, and since only short exposures can be given during the brief duration of totality, the spectra have to be photographed on a comparatively small scale. The positions of these lines cannot therefore be determined with anything like the accuracy attained in most spectroscopic measurements; and this greatly enhances the difficulty of their identification. They certainly do not correspond to the lines of any of the known atoms existing under the physical conditions that can be produced in the laboratory. On the other hand it is practically certain that they are due to quite ordinary atoms in an abnormal state, just as the nebular lines have been found to be. Dr. Bowen identified the nebular lines by showing that their positions in the spectrum corresponded exactly with those calculated from theory for certain of the forbidden lines of the variously ionized atoms of oxygen and nitrogen. But the attempts of himself and others to do the same thing for the coronal lines has not yet met with success. Recently Drs. Menzel and Boyce at Harvard have obtained evidence indicating that many of the brighter coronal lines are due to unionized oxygen. But there are appreciable discrepancies between the positions of the oxygen lines and the best determined positions of the coronal lines. Still better measurements of the coronal lines may, it is true, remove these discrepancies; but until that is done one cannot accept the identifications with any degree of certainty. Undoubtedly it is in the further development of M. Lyot's technique for photographing the coronal spectrum in daylight that, as regards this particular problem, our greatest hope for the future lies.

The corona, at least in some respects, is closely analogous to the gaseous nebulae. Both shine partly by light reflected from small particles and partly by light emitted from atoms whose electrons are executing jumps that are impossible except in excessively rarefied gases. In both the emission of the bright lines is made possible by the energy fed into the atoms by the radiations of a hot body—the sun or the involved stars. Perhaps, as we have suggested, the analogy may be even closer, the corona being a gaseous nebula in miniature. It is possible that just as the intense radiation pressure of the O and B stars ejects large quantities of material to immense distances, the comparatively weak radiation pressure of our G-type sun ejects small quantities to small distances; and that whereas the radiations of the O and B stars excite the atoms to emit the nebular lines, those of the sun excite perhaps similar atoms to emit the coronal lines.

Though we do not know why the " electron gas " of the corona assumes the form of long curving streamers, there is no doubt at all that the general arrangement of these streamers is intimately connected with the spot cycle. It has been suggested that the shapes of the streamers represent the shapes of magnetic lines of force; and in that case one might expect them to be determined to some extent by the intense local magnetic fields in the spot zones. Perhaps that is why at minimum spot activity, when the two zones are confined close to the equator, we get the characteristic minimum corona with its symmetrical equatorial streamers; and why near maximum activity, when the two zones are in higher latitudes, we get the maximum type of corona with its streamers scattered more irregularly round the sun.

At the 1919 eclipse two British expeditions confirmed Einstein's prediction that light is deflected from a straight line by a gravitational field. Since then the measurement of this deflection by methods of ever-increasing accuracy has taken an important place in many eclipse programmes. A brief account of this matter, which belongs entirely to the realm of relativity, has been given on pp. 143-146.

Very large scale photographs are often required at eclipses, and this entails the use of very long and cumbersome telescopes. Two methods of mounting such instruments are commonly employed. In one the telescope is fixed horizontally, while the sun is fed into it by a coelostat, a form of rotating mirror first used by Turner of Oxford in 1896. In the other, which was invented by the American astronomer, Schaeberle, in 1893, the telescope is pointed directly at the eclipsed sun by supporting its upper end on a high tower, while the photographic plate is moved by clockwork. In both methods the telescope itself is stationary, the earth's rotation being counteracted during totality by the motion of the mirror or the plate. Telescopes up to 65 feet in length are now quite commonly used at eclipses, and on a few occasions one with a length of 135 feet has been employed with success.

CHAPTER XXV

THE PLANETS

UNTIL quite recently the only way to study the physical conditions of the planets was to observe them directly with the telescope: to make careful drawings of their surfaces, to measure the positions of the various markings and to note their colours and intensities, and to follow in detail from hour to hour, night to night, and year to year any changes that might take place. The method is very laborious, and the results often emerge only after the passing of many years. Yet despite the development of other methods which now supplement it, it is still indispensable and must always remain the principal method upon which the advance in planetary knowledge depends.

Long ago Huggins and other pioneers of the spectroscopic method had examined the spectra of the planets and found them to be, at least very closely, replicas of the solar spectrum. This was what was to be expected, since the planets are non-luminous and shine only by reflecting the light of the sun. But if a planet possesses an atmosphere it might be supposed that the sunlight in its passage through that atmosphere would be enriched by the additional lines of the gases composing that atmosphere. This happens in the case of our own atmosphere, which superposes upon the solar spectrum proper certain lines produced by the oxygen and water vapour within it. Such terrestrial lines are however to be distinguished from the solar lines by the fact that they grow more intense as the sun sinks lower in the sky and is seen through a greater thickness of air.

In the spectra of the three inner planets Huggins had found no evidence of such additional lines, but in the spectra of Jupiter and Saturn he had found a few additional broad bands which he was unable to identify. Somewhat later exactly similar broad bands were found in the spectra of Uranus and Neptune; but still the origin of them remained a mystery. And in this somewhat unsatisfactory state the spectroscopic study of planetary atmospheres remained for a great many years.

The problem is of necessity a very difficult one, especially if we are looking for lines due to absorption in atmospheres the composition of which is similar to our own. The reasons are these:

(1) The main constituents of our atmosphere are nitrogen, oxygen, carbon-dioxide and water vapour. Nitrogen gives rise to no lines in the observable parts of the spectrum, and the lines of the other three gases are many of them weak or difficult to observe. (2) If the atmosphere of a planet is rarefied in comparison with our own, the lines due to the above gases will be very difficult to distinguish from the same lines due to our own atmosphere. (3) Even though a planet may possess a comparatively dense atmosphere, the sunlight might be prevented from penetrating it to any appreciable depth, by being reflected from a high-lying layer of opaque cloud. And so it was on the face of it very unlikely that the search for these gases in the atmospheres of the planets would succeed until powerful spectrographs were available and until a great deal of preliminary work had been done in developing a suitable technique.

It was Professor Slipher who first fully appreciated these points and started at the Lowell Observatory over thirty years ago the systematic work on planetary spectra which he has carried on ever since. One problem was to show up the possible presence in the planetary atmospheres of gases similar to those in our own atmosphere, in spite of the fact that their lines would be superimposed on the corresponding terrestrial lines. His method was to photograph on the same plate a spectrum of the planet and a spectrum of the moon—which we definitely know possesses *no* atmosphere—and to repeat the photographs frequently as the two bodies rose higher in the sky. The oxygen and water vapour lines in the moon's spectrum, which must be *entirely* due to our own atmosphere, would grow weaker as the moon rose higher above the horizon. If the planet's atmosphere contained neither of these gases, then the series of planetary spectra would be identical with the series of lunar spectra. But if the planet's atmosphere did contain these gases, then the corresponding lines in its spectra would, with increasing height above the horizon, fall off in intensity less rapidly than the same lines in the lunar spectra.

By such methods Professor Slipher has done an immense amount of work on the spectra of Mercury, Venus and Mars; and at the same time he has made detailed studies of the spectra of Jupiter, Saturn, Uranus, and Neptune, discovering several new bands in the red and infra-red regions. More recently the investigation of planetary spectra has been carried on by the Mount Wilson observers with their still more powerful equipment.

It has lately been possible to measure the temperatures of the planetary surfaces. A body like a planet will be heated by the sun's radiation to a definite temperature. Knowing the distance of the planet from the sun this temperature can be calculated within certain limits which depend on the physical properties of the planet's surface. But if a planet is warm—either as a result of the heat received from the sun or as a result of any internal heat of its own—it must itself radiate heat into space. Since the intensity of this planetary radiation gives us a measure of the temperature of the planet's surface, it would be possible, if we could measure the intensity of the radiation, to obtain a direct check on the calculated temperature of the surface. If it was found that the measured temperature of a planet was much higher than its calculated temperature, we should be able to conclude that the planet still possessed heat of its own and that its surface had not yet completely cooled down like the earth's surface.

The measurement is made by means of a " thermo-couple," upon which is focused the image of the planet formed by a large telescope. The thermo-couple, as we saw in a previous chapter, measures the intensity of the radiations in *all* parts of the spectrum, and thus gives the total radiation coming from the planet. But much of the radiation measured in this way is simply that of sunlight *reflected* from the planet's surface; and this must somehow be excluded from the measurements before we can use them to give us the planet's temperature. Fortunately, since the temperatures of the planets must be comparatively low, the planetary radiation proper must be of long wave-length and will be situated in the infra-red part of the spectrum which is completely blotted out by a thin layer of water. One therefore has to make a second series of measurements by allowing the light of the planet to pass through a small vessel containing water before it reaches the thermo-couple. The measurements made through the water will thus include only those radiations that are simply *reflected* from the planet. In this way one can determine what fraction of the total radiant energy, measured in the absence of the water, is simply *reflected* by the planet and what fraction is actually *radiated* by the planet. It is this second fraction that gives us the temperature of the planet. These measurements, which are very difficult to make, have been successfully carried out at the Lowell Observatory by Drs. Coblentz and Lampland and later at Mount Wilson by Drs. Pettit and Nicholson.

Mercury. The conclusion of Schiaparelli and Lowell that Mercury rotates on its axis in the same period as that in which it revolves round the sun, namely, eighty-eight days, has been amply confirmed. Mercury thus keeps always the same face to the sun just as the moon keeps always the same face to the earth. In the case of the moon we saw how this result had been brought about by the tides raised on the moon by the earth. Undoubtedly the solar tides have in the same way slowed down the rotation of Mercury. The surface of Mercury has been very carefully studied by M. Antoniadi with the 33-inch refracting telescope at Meudon. Though his chart of the planet shows greater detail than that of Schiaparelli made with a much smaller telescope, the two are in good agreement. He also confirms Schiaparelli's suspicions of a thin atmosphere, for he finds that certain of the surface markings are occasionally obscured as though by dust. Apart from this he finds the markings are permanent and do not change in appearance. It seems pretty certain that the markings are due to irregularities of the surface; and it is possible, as Barnard long ago suggested, that the Mercurial landscape is not unlike the mountainous landscape of the moon. Since Mercury always turns the same side to the sun, one hemisphere will be intensely hot and the other intensely cold. This has been confirmed by the direct temperature measurements, which show that in the daylight hemisphere the temperature is above 400 degrees centigrade. As to the composition of the thin Mercurial atmosphere we know nothing, for no spectroscopic evidence of it has been found.

Venus. The rotation period of Venus has been the subject of much controversy, for the markings that are seen upon the planet are extremely indefinite. Some observers have concluded that the period is about twenty-three hours, the length of Venus' day being very similar to that of our own. Others, like Schiaparelli, believed that Venus, like Mercury, always turned the same side to the sun and rotated and revolved in a period of 225 of our days. The latest observations of M. Antoniadi show that the period is a very long one, but is not necessarily so long as its period of revolution round the sun. A similar result has been obtained by Dr. Slipher, who has attempted to measure the shift in the spectral lines from opposite sides of the planet's disc. No shift could be detected, and he was able to show that the rotation period must be at least several weeks long. Since Venus is much further from the sun, the action of the solar tides in slowing down its rotation would have been much less effective than in the case of Mercury. So it is quite possible that the slowing down process on Venus, though well advanced, is still incomplete.

It used to be supposed that the dusky markings on Venus represented rifts in the clouds through which we saw down to the planet's surface. M. Antoniadi however has come to the conclusion that though the dusky areas are related to the character of the underlying surface, they are due to a thinning, rather than to a complete break, in the cloudy strata.

When Venus is between us and the sun and appears as a delicate crescent, it is found that the horns of the crescent, instead of being limited to a semicircle as in the case of the moon, may extend considerably further round the circumference of the planet. This must mean that there is a region of twilight on Venus just as there is on the earth, and that a portion of the planet on which the sun is not directly shining is faintly illuminated by the light scattered in its atmosphere. Thus quite apart from the observations of clouds there is evidence of an extensive atmosphere on the planet. In spite of this, however, it was not until 1932 that any spectroscopic evidence for an atmosphere was obtained. In that year Professor Adams and Dr. Dunham found at Mount Wilson certain bands in the infra-red indicating the presence of considerable amounts of carbon-dioxide. There is no indication of oxygen or water vapour. But since the light analysed by the spectroscope is probably reflected from clouds high up in Venus' atmosphere, we cannot be too dogmatic about the absence of such gases from the parts of the atmosphere beneath the clouds.

Mars. The earlier observations of Mars have been discussed in a previous chapter: viz., (1) The discovery of the bright and dark markings on the surface, which were more or less permanent and indicated that the planet rotated on its axis in a period of twenty-four hours and thirty-seven minutes. (2) The discovery of a white cap at either pole of the planet, which waxed and waned in size with the planet's seasons thus suggesting an analogy with the polar snow-caps of the earth. (3) Schiaparelli's discovery of certain dusky streaks crossing the bright parts of the planet, which he called *canali* or " channels " but which now, through frequent misusage of the word, are called " canals." (4) Lowell's discovery that the dark markings, which were previously thought to be seas, were also crossed by the canals; and his suggestion that they should therefore be regarded as areas of vegetation in contrast with the bright areas which should be regarded as deserts.

During the last forty years a great deal of data has been laboriously collected; and it is now possible to draw certain pretty definite conclusions as to the nature of the Martian surface and as to the physical conditions existing there.

There is now practically no doubt that the pole caps of Mars are composed of snow. The regularity with which they dwindle in size during the summer and grow large again in the winter makes it certain that they consist of some substance which vaporizes when the temperature rises and is precipitated again when it falls. Practically the only other white solid substance known that could behave in this way is carbon-dioxide. But solid carbon-dioxide. vaporizes at a much lower temperature than snow or water does; and direct measurements show that the temperature changes occurring on Mars, while perfectly compatible with the existence of snow, practically exclude the possibility of solid carbon-dioxide. Often bright white areas are seen obscuring portions of the surface upon which the sun has just risen. The whiteness tends to disappear as the Martian noon approaches and to reappear again towards the late afternoon. It is very probable that many of these white patches represent hoar-frosts; for it is found that in the early Martian morning and late Martian afternoon the temperature is far below freezing-point, while towards noon and in the early afternoon the temperature is well above freezing-point and may exceed fifty or even sixty degrees Fahrenheit. (It is possible that at the very low atmospheric pressure which probably exists on Mars, the snow might often evaporate to form water vapour in the atmosphere without passing through the intermediate liquid state.)

The dark markings on the planet appear to undergo changes of two kinds, changes which appear to be seasonal and changes which apparently are quite irregular. Lowell concluded from his observations that the seasonal changes were remarkably simple. As the snow-cap of the summer hemisphere melted, so a wave of darkening was believed to spread equatorwards affecting successively the various dusky markings in its path. During their darkening the colour of the markings would change from a light to a darker green and later to brown and yellow. The work of other observers shows that the changes which occur are by no means so simple and straightforward; in fact many have doubted whether any evidence of seasonal changes exists. The observations of M. Antoniadi with the great Meudon telescope have, however, shown that there are complex changes in the colours and outlines of certain of the bright and dark regions which are definitely seasonal in character. But the sequence of events as it affects different parts of the planet's surface is by no means so simple as Lowell supposed. The problem is a most intricate one, and its further elucidation is probably the most important task with which observers of the planet are faced. It must be remembered that the earth overtakes and approaches Mars at intervals of about two years and two months; that at such times Mars is near enough for satisfactory observation for only a few months, and at most for about one-sixth of its own complete cycle of seasons; and that in order to observe Mars through one complete cycle of its seasons fifteen years is required. Finally, it must be realized that during the few months that Mars is near to the earth, there are often not more than half a dozen nights on which our own atmosphere is sufficiently steady for the finest details on the planet to be seen.

The seasonal changes that have been definitely established, together with the vivid colours which are revealed by large telescopes in the dusky areas, leave practically no doubt that these parts of the planet's surface are covered by some sort of vegetation.

The changes that are not seasonal are sometimes completely irregular and are sometimes repeated at irregular intervals. They may take the form of marked alterations in the colour and intensity of some localized region, or of a complete metamorphosis in the shape of a dark marking. Thus *Nepenthes-Thoth* which is a curved dusky streak over 700 miles long became enormously widened and darkened in 1888 and again between 1911 and 1929. Another dusky marking called the *Solis Lacus*—for the classical nomenclature based on the former belief that the dark regions represent water is still retained—underwent an extraordinary transformation between 1924 and 1928. In 1924 it resembled, as in all previous years, a dark elongated blob, about 500 miles long and about 300 miles wide. When the planet approached again in 1926 the *Solis Lacus* had extended much further to the north, and was shaped like a carrot with its point directed towards the equator. At the planet's next return in 1928 the *Solis Lacus* was found to have resumed its normal appearance. Such phenomena may be due to temporary modifications in the growth of vegetation possibly produced by climatic variations. Changes of this kind, however, are rare and repeat themselves, if at all, only at long intervals. So until Mars has been observed through many more complete seasonal cycles—until several generations of astronomers have come and gone—their full significance is not likely to be understood.

The canals of Mars have given rise to a vast amount of controversy. There is no doubt as to their existence, or at least as to the existence of the more conspicuous of them. They can be seen with both large and moderate-sized telescopes, and the strongest of them have been photographed. The question at issue is in regard to their appearance. Lowell with the 24-inch refractor of his observatory saw them as perfectly straight and very narrow, sharply defined lines forming an almost geometrical network upon the planet's surface. Barnard who observed with many of the world's largest telescopes and M. Antoniadi who has studied Mars for many years with the 33-inch refractor at Meudon, find the surface to be covered by irregular and diffuse shadings which in places take the form of hazy streaks. But these streaks vary considerably in width and are often seen to be broken up and discontinuous. With this impression the great majority of experienced observers, using telescopes from ten to twenty-eight inches in diameter, would agree. A few, however, do not, and claim that their observations confirm those of Lowell.

The writer when he first started observing Mars found that he was inclined to see more or less linear detail upon the disc. But as time passed that tendency grew less and less, and he began to see the detail more nearly like what he now considers to be its actual appearance. It undoubtedly takes many years for the eye to become trained for the appreciation of the finest telescopic detail. Before that is accomplished it is liable to see hazy, discontinuous streaks as narrow and continuous lines, to interpret a complex system of light and dark shadings as a more or less linear geometrical pattern, and even to join up the larger and quite obvious markings by lines which do not possess any objective reality. The writer finds that this tendency is still liable to recur when the planet is far from the earth and difficult to observe, when our atmospheric conditions are poor, and when telescopes of smaller size are used. Under normally good conditions he sees the canals as wide and diffuse streaks which often form the border of a more extensive shading and are generally irregular in width and sometimes discontinuous. Finally, under the very best atmospheric conditions—which occur only momentarily on comparatively few nights in the year—some of these streaks remain while others melt away into a background of more complex structure.

The majority of Martian observers would agree that the tendency to see the canals as approximately geometrical lines is due to one or other, or to a mixture, of the following causes. (1) The eye may be incapable of appreciating the finest telescopic detail. This failure is less often due to imperfections in the optical qualities of the eye than to an inadequacy of the nervous mechanism through which the brain interprets the optical image. (2) The failure on the part of the observer to be sufficiently critical of his " first impressions." It may be necessary to watch through many nights in order to catch the brief intervals of the best atmospheric conditions. (3) The excessive rarity of good atmospheric conditions in certain localities. (4) The use of telescopes of insufficient size.

Apart from the question of seeing there is the question of drawing. All drawings are to some extent conventional or diagrammatic. With the best draughtsmen this conventionality is reduced to a minimum, and their drawings give an approximately perfect representation of the planet. But a first-rate observer may be a poor draughtsman and be forced to make drawings that are largely diagrammatic. Up to a point this does not matter and for many purposes a partly diagrammatic drawing is all that is necessary. But the danger is that a poor draughtsman may be unconscious of the extent to which his drawing is diagrammatic. In attending to the detail one may miss the general impression of the view as a whole. Since the finer spots and streaks on Mars are very difficult to see, one is apt to overestimate their smallness and narrowness when one comes to draw the planet, as one must do on a much larger scale than one sees it. Thus it may easily happen that failing to realize that the drawing is largely conventional, one bases one's conclusions as to the appearance of Mars, not on what one has actually seen, but on what one has drawn.

It might have been supposed that photography could have provided us with a final and impersonal judgment on this controversy. Unfortunately it helps not at all. The detail under consideration is so minute that even in the images of the planet formed by the largest telescopes much of it is finer than the grain of the photographic plate. Further, the slight tremors that are present in our atmosphere on the steadiest nights are sufficient to obliterate during the exposure all but the very coarsest canals from the photograph. Thus though Dr. Slipher has secured many very beautiful photographs of Mars and the other planets with the large telescope at the Lowell Observatory, they show less detail than can be *seen* with comparatively small instruments.

The evidence for the existence of a Martian atmosphere was, we saw, beyond dispute. Thus there are the clouds that are sometimes seen obscuring the surface; and there is the behaviour of the snow-caps and patches of hoar-frost which could only be possible in the presence of an atmosphere. A still more conclusive piece of evidence, also noted by the earlier observers, is that the disc of the planet appears brighter towards its edge, and that when the dark markings are carried by the rotation of the planet into this bright peripheral zone they are more or less obscured. This can only be due to an atmospheric envelope: towards the edge of the disc we must be looking through a deeper layer of it, which will scatter the sunlight more brightly and obscure to a larger extent the underlying surface.

The clouds are of two kinds, the yellow clouds and the white clouds. The yellow clouds are the more frequent, but as their colour approximates to that of the surface they are less easy to detect. Occasionally these clouds are enormous, obliterating or partially obliterating the larger part of a whole hemisphere; and they may persist for many weeks. It is possible that they are dust clouds raised from the Martian soil. White clouds are much rarer and seldom last more than a few days. They are very opaque and sometimes show a flocculent structure like a mass of cumulus cloud seen at a great distance. Measurements of the positions of the clouds from day to day have indicated wind-velocities on Mars ranging from about ten to twenty miles an hour. Occasionally clouds near the edge of the disc may be seen actually projecting beyond it, so that their heights above the Martian surface can be estimated. In 1924 M. Antoniadi observed a projecting mass of cloud which attained the remarkable height of about twelve miles.

An interesting investigation of Mars' atmosphere was carried out by Dr. Wright at Lick in 1924. He took photographs of the planet in violet and deep red light and found remarkable differences. The picture in violet light, which consisted mainly of light reflected from the particles in Mars' atmosphere, showed very little of the surface detail, being to all intents and purposes a photograph of the planet's atmospheric envelope. The picture which was taken in red light, however, showed the dark markings very clearly, for red light is only slightly scattered by an atmosphere and would thus reach and be reflected from the surface itself. The diameter of Mars in the violet picture is considerably larger than it is in the red picture, indicating that the highest and most rarefied parts of the atmospheric envelope reach to fifty or sixty miles above the surface. An interesting feature of the violet pictures of the Martian *atmosphere* is that they show what are apparently the polar snow-caps. But this does not mean that our deductions as to the nature of the pole-caps were erroneous; it simply indicates that high-lying mist or cloud tends to collect above these regions. The direct visual observations have for a very long time pointed to the same conclusion; for the dazzling whiteness of the snow cap itself is often obscured by the comparatively dull whiteness of overlying cloud or haze, and this is particularly liable to happen at a time when the snows are melting rapidly in summer.

But though Mars has undoubtedly a considerable atmosphere extending to a great height, there is but little evidence as to the density of that atmosphere. On general grounds it is probably less dense than our own; for since the force of gravity on Mars' surface is but one-third of what it is on the earth's surface, the diffusion of its atmosphere into space would have progressed more rapidly than that of our own. The extent of the twilight into the night hemisphere is less on Mars than on the earth, which also indicates that Mars has a more rarefied atmosphere than the earth has; but this measurement is very difficult and the results are none too certain. We cannot, as might be thought, draw any reliable conclusions from the infrequency of the Martian clouds. That is already accounted for by the great scarcity of water on the planet; for we know of no expanses of open water on Mars and can be certain that none exists to compare in size, for example, with our own Mediterranean. From the height at which meteors first become visible through being heated to incandescence by the friction of the air, we know that our own atmosphere must reach a height of at least 100 miles. But that does not mean that violet and red photographs taken from Mars would also indicate a depth of 100 miles for the earth's atmosphere. The result might be more or less; so we cannot draw any conclusions from the photographs of Dr. Wright as to the *density* of Mars' atmosphere.

So far the spectroscopic observations bearing on the composition of the Martian atmosphere are negative. At one time both the Lowell and Mount Wilson observers thought they had detected traces of oxygen and water vapour. But more recently they have concluded that there is no certain indication of either of these substances, and that their amount above a given area of the Martian surface is less than one per cent. of their amount above a similar area of the earth's surface. Since the development in 1932 of the method which led to the detection of carbon-dioxide on Venus, the apparitions of Mars have not been favourable for so delicate an observation. So far the gas has not been found; but too much significance should not be attached to this negative result until the observations have been repeated at the more favourable apparition in 1941.

We may thus draw the following conclusions.

(1) Mars has a considerable atmosphere which is probably less dense and may be very much less dense than our own.

(2) The temperatures are much lower than on the earth. Only round the middle of the day does the temperature rise above freezing and it never much exceeds fifty or sixty degrees Fahrenheit.

(3) There are no open seas on Mars. The only water which can be directly seen is in the form of snow, frost or cloud.

(4) It is extremely probable that there is some form of vegetation on Mars.

(5) There is no evidence whatsoever for the existence of intelligent life.

(6) The negative spectroscopic evidence as to the composition of the atmosphere has an important bearing on the question of life on Mars. The failure to detect water-vapour is doubtless due to the smallness of the amount diffused in the general atmosphere. In the snow and the clouds water is in the solid or liquid form and would not produce spectral lines. It is conceivable that our only hope of detecting water-vapour would be to carry out the excessively delicate operation of photographing the spectrum of the narrow edge of the melting snow-cap. Despite the negative spectroscopic evidence we can however be certain that water-vapour exists in small amounts in the atmosphere—in amounts probably sufficient for the needs of some hardy form of vegetation. The spectroscopic evidence that oxygen exists, if at all, only in the smallest amounts seems indisputable, and practically excludes the possibility of animal life. For animal life, as opposed to vegetable life, is dependent on an atmosphere of oxygen of which the pressure cannot be very much inferior to that of the oxygen at sea-level on the earth. On the other hand an essential property of vegetable life is to be able to take up carbon-dioxide from the atmosphere and, after retaining carbon, to return oxygen back to it; and certain forms of vegetation can exist by this process in the complete absence of free oxygen. For the presence of carbon-dioxide the evidence is so far inconclusive, and it is still possible that it may be found to exist in as large amounts as it does on Venus. Yet even if it is shown not to be present in amounts sufficient for spectroscopic detection, the result would not be incompatible with the existence of vegetation; for in our own atmosphere the amount of carbon dioxide is extremely small.

The interest shown in the question as to the possibility of life on Mars is usually centred on the possibility of *intelligent* life. But really the interesting point is whether there is life of *any kind.* For the gap between vegetable life and animal life, intelligent or otherwise, is small compared with that between the animate and the inanimate world. Thus the conclusion that Mars most probably possesses vegetation will, if indisputably confirmed, be of the very widest significance. It would seem to indicate that the appearance of life on a planet, instead of being the result of a unique accident or special dispensation, is the result of certain well defined processes which given suitable conditions will produce life automatically.

Jupiter. Beyond Mars come the minor planets; but we shall pass these over for the moment and go on to consider the next planet, Jupiter, which is the largest of all the planets. The dark belts that are seen encircling the globe of this planet are roughly parallel to its equator and are separated from one another by bright zones. The two most conspicuous belts lie on either side of the bright equatorial zone, while in higher latitudes there follow a succession of zones and belts alternating with one another.

The structure of the belts and the detail exhibited by the brighter and darker markings seen in the bright zones are often extremely intricate and may change rapidly. The colours of the belts are sometimes very vivid. Usually the belts have various shades of red, brown and orange, but they may show a definitely bluish tinge.

It had long been known that Jupiter did not rotate as a solid body, and that like the sun its equatorial regions had a shorter rotation period than the parts of its surface in higher latitudes. But it was soon found that its resemblance to the sun in this respect was only superficial: instead of the rotation period increasing evenly towards higher latitudes, the variation was quite irregular and often punctuated by abrupt discontinuities. All that could be said was that the regions in the immediate vicinity of the equator rotated in a period of about nine hours fifty minutes, which was, roughly speaking, five minutes shorter than that in which the rest of the planet rotated.

The man who really founded the systematic observation of Jupiter and first brought order into this apparent chaos was Mr. Stanley Williams, an amateur astronomer who is among the most eminent of planetary observers. His classical paper to the Royal Astronomical Society in 1896, " On the Drift of the Surface Material of Jupiter in Different Latitudes," established the existence of a number of currents in the various parts of the different zones and belts, which determine the rotation periods of the spots seen on the planet. Since then the telescopic observation of Jupiter has been mainly concerned with the determination of the rotation periods of all markings that are sufficiently conspicuous and permanent for them to be identified from night to night. By far the greater part of this important work has been carried out by the members of the Jupiter Section of the British Astronomical Association and particularly by the Rev. T. E. R. Phillips, who directed the work of the section for over thirty years. In this way it has been shown that the eleven currents originally established by Mr. Stanley Williams, are permanent features of the Jovian atmosphere; while at the same time other currents have been discovered some of which are liable to be periodically suppressed and then reappear after the lapse of several years. With one exception these numerous currents flow almost entirely in longitude, the spots belonging to them drifting in a direction parallel to the equator. But the observations of the Rev. T. E. R. Phillips and Messrs. Hargreaves and Peek have shown that in 1920, and again in 1931 to 1934, there were some small spots in a particular part of the southern hemisphere whose motion indicated a *circulating* current rather like a very elongated anticyclone.

There is of course no doubt that all the markings visible on the disc of Jupiter are atmospheric phenomena, and consist of clouds or vapours floating in a dense Jovian atmosphere. Apart from their drifting relatively to one another, the markings are continually changing in shape. Some of these markings are quite transient; but the majority persist for many days or weeks, and occasionally for many months. There are two markings, however, that are quite unique as regards their persistence: the Red Spot which has been visible since 1878, and the South Tropical Disturbance which has been under observation for over thirty years. The Red Spot has had various ups and downs: sometimes it has been very conspicuous, sometimes its colour has almost left it so that it could barely be seen. Denning, from an examination of the observations of Jupiter back to the earliest times, was able to show that the Red Spot was definitely to be identified with a marking observed by Schwabe in 1831, and was possibly the same as Hooke's spot in 1664. The South Tropical Disturbance first seen in 1901 is in the same latitude as the Red Spot; owing to its more rapid rotation round Jupiter it overtakes the Red Spot at intervals which at first were about two years in length but have since become very much longer. When this happens the disturbance, in passing the spot, tends to pull the spot along with it, and then to let it drift back again to its previous position. But though the Red Spot has remained visible for so long it is not, as might be thought, a part of the solid underlying surface seen through a break in the clouds; for the rotation period of the Red Spot is subject to very large and irregular variations.

The bright and particularly the reddish colours seen in the belts were at one time considered to be due to glowing vapours rising into the atmosphere of a planet that was still in a hot molten condition. But direct temperature measurements have shown that the layers of the Jovian atmosphere which we see are about 130 degrees centigrade below freezing. Within the limits of error of the measurement this temperature is roughly the same as that which the planet would have if its own internal heat was negligible and it was heated entirely by the sun. We can be certain, therefore, that the colours are not due to glowing vapours; but it is still possible that the planet has a small amount of internal heat, and that the disturbances which occur in its atmosphere on such a gigantic scale are the result of the lower atmospheric layers being appreciably warmer than the upper layers.

From theoretical considerations based on the measured density of Jupiter, which is about 1·3 times that of water, and on the degree of polar flattening produced by its rapid rotation—for Jupiter shows very marked flattening like an orange—Dr. Jeffreys suggests that the planet has a rocky central core covered by a thick layer of ice and surmounted by an extensive atmosphere.

As we shall see in a moment the atmosphere of Jupiter, like those of the three planets beyond him, has recently been shown spectroscopically to contain large quantities of ammonia and methane (Marsh gas or Fire Damp); we shall also see that it is probable that the only other important constituents of this atmosphere are hydrogen and helium. This discovery, however, throws no light on the colours seen in the Jovian clouds; they still remain a complete mystery. Another important question which is raised but not answered by the visual observations has reference to the levels in the atmosphere at which the various clouds and currents are situated. Does, for example, the velocity of a current depend at all on its depth within the atmosphere? Is it not possible, also, that the colours of the clouds are to some extent determined by their level in the atmosphere? It is quite likely that valuable information on such points is to be got by the method, used in the case of Mars' atmosphere, of photographing the planet in light of different colours. In the last few years Dr. Wright at Lick has obtained very fine photographs of Jupiter in light from various parts of the spectrum from the ultra-violet to the infra-red; and it is hoped that, if such photographs can be made in combination with the visual observations, we may get some indication as to the levels at which the various atmospheric phenomena are situated.

Saturn. The globe of Saturn, as was seen in our discussion of the earlier observations, appears not unlike that of Jupiter. It is considerably more flattened at the poles than is the case with Jupiter. It has two dusky belts bordering a bright equatorial zone, and occasionally there are other dusky belts in higher latitudes. But the belts and zones are much less conspicuous than those on Jupiter, and it is only very rarely that any irregularities appear in their outlines or that any spots are to be seen upon them. Between 1794 and 1891 three sets of equatorial spots had been studied sufficiently accurately to give the equatorial rotation period. This was found to be closely ten-and-a-quarter hours.

It was not until 1903 that reliable observations were obtained of spots in higher latitudes. In that year Barnard observed a spot about forty degrees north of the equator giving a rotation period of ten hours thirty-eight minutes. The Rev. T. E. R. Phillips found a very similar period for a spot he observed in the same latitude south of the equator in 1910. Some dark spots in intermediate latitudes studied by Mr. Butterton in 1932 gave, as might be anticipated, a rotation period between that of the higher latitudes and that of the equatorial regions. In 1933 the most conspicuous spot ever seen on Saturn was discovered by Mr. W. T. Hay in London. A very large number of observations were made in all parts of the world of this spot and also of a second one, discovered at the Lowell Observatory, which immediately followed it. Both were equatorial spots and their rotation periods agreed closely with those of all previous equatorial spots since Herschel's spot in 1794. One can conclude that the rotation period remains constant at the equator over long periods; and that it increases in length in higher latitudes much more markedly than in the case of Jupiter. We cannot tell however whether its variation with latitude is regular, or irregular and discontinuous: whether in this respect Saturn resembles the Sun or Jupiter.

The temperature measurements of Saturn come out much the same as those of Jupiter; again, one cannot be certain whether Saturn has no internal heat or whether it still retains a little. Probably its internal condition is similar to that of Jupiter; and, as we shall see later, the atmospheres of the two planets differ in their composition only in detail.

We saw how Keeler in 1895 had proved spectroscopically that the rings of Saturn were composed of myriads of minute particles revolving round the planet in their individual orbits. In that case the rings, if sufficiently thin, should be to some extent transparent to light, the extent depending on their thinness and the closeness with which the individual particles are packed. Of the three rings of the planet the middle one is very much the brightest, its surface being at least as bright as that of the planet itself. The outer ring is markedly fainter than the middle one; and the inner, or crêpe ring is extremely faint and only visible in moderately large telescopes. It is probable that the brightness of the various rings—their power to reflect sunlight—is mainly dependent on the closeness with which their component particles are packed: the closer the packing the brighter the ring. Thus one might have predicted that the middle and brightest ring would be the least transparent, and the inner and faintest ring the most transparent. This has been confirmed in a number of ways.

It had long been known that the faint inner, crêpe ring was largely transparent; for where it crosses in front of the planet the outline of the planet's globe is clearly seen through it. For a few months every fifteen years the rings are presented edge-on to the earth; and for part of this time we look down upon their dark side, the side opposite to that on which the sun is shining. On such occasions certain remarkable phenomena are to be seen; and these were studied with great care by Barnard in 1907. The appearance then is that of a very fine needle projecting from either side of the planet, the needle being brightest near to the planet and again in two places at some distance from its two extremities. The brightest portions of the rings seen under these conditions thus correspond to the position of the crêpe ring and to the position of the " Cassini Division," which is the gap between the outer and the middle rings. Though the illumination of the " dark " side of the rings must be due in part to light reflected on to it from the planet, the greater part of it must be the result of the sunlight filtering through them from their sun-illuminated side. Thus it is the least densely packed crêpe ring and the Cassini Division which, by letting through and scattering the most sunlight, appear brightest. On the other hand we cannot conclude from the faint illumination of the outer and middle rings seen under these conditions, that they also have some degree of transparency, for it may be that their illumination comes entirely from light reflected on to them by the planet.

In 1889 Barnard observed Iapetus, one of Saturn's satellites, undergo eclipse by passing into the shadow cast by the planet and its rings. The light of the satellite was completely obliterated while it was passing through the shadow of the middle ring and the shadow of the planet, but was faintly seen while in the shadow of the crêpe ring. The transparency of the outer ring was first proved by a very beautiful observation in 1917 by Captain M. A. Ainslie and Mr. Knight. A star of the 7th magnitude was seen to approach and pass behind the outer ring and later to emerge in the gap between the outer and middle rings. After travelling along the gap, or Cassini Division, it again passed behind the outer ring and finally emerged at its outer edge. The star therefore was never eclipsed by the middle and brightest ring. During its second passage behind the outer ring it was clearly though faintly seen; and twice during this time it momentarily brightened up. On emerging in the Cassini Division it regained its full brightness. This showed that the outer ring is moderately transparent and that in places it is more transparent than in others. The proof of the transparency of the middle and brightest ring was obtained on March 14, 1920. On that night W. Reid, the South African amateur astronomer and the discoverer of several comets, observed a star pass as far as he could tell behind almost the entire breadth of the middle ring. Although it underwent several rapid fluctuations in brightness its light was never completely obliterated.

The thickness of the ring, which as we have said must be excessively small, can be investigated when the rings are edgewise to the earth. This happened in 1907, 1921 and 1937, but on the last occasion the planet was too near the sun for critical observation. On the two earlier occasions, however, excellent observations were secured with some of the largest telescopes in the world. For a day or so on each occasion the rings entirely disappeared; and from this it can be concluded that they are certainly not more than seventy miles thick and probably not more than ten miles thick.

Uranus, Neptune and Pluto. The five classical planets have been known from the earliest times and were never in the ordinary sense of the word "discovered." But the three most distant planets, which we shall consider together, have all been added to the solar system within the last 160 years. The circumstances of the discovery were completely different in each case. Uranus was discovered in 1781 by Herschel while conducting a preliminary survey of the heavens with a comparatively small telescope. He noticed at once the peculiar appearance of a particular star, whose image looked slightly larger than that of an ordinary star. But with the small telescope which he was using the difference must have been very slight, and only a most gifted observer could have detected it. The discovery of Neptune was a stupendous achievement of mathematical theory. We have seen how independently Leverrier and Adams from the irregularities in the motion of Uranus predicted correctly the position in the sky of the hypothetical planet responsible for them.

Pluto was discovered in 1930 under remarkable circumstances. For many years attempts had been made to detect irregularities in the motion of Neptune or further irregularities in the motion of Uranus that might lead to the discovery of a trans-Neptunian planet. The chief investigation was made by Lowell in 1915. Since Neptune has been under observation a comparatively short time, any irregularities in its motion are bound to be very uncertain. It was, therefore, necessary to confine the investigation to such irregularities in the motion of Uranus as had remained unexplained by the discovery of Neptune. These, however, turned out to be extremely small and indefinite, and incapable of giving anything like the precision achieved by Leverrier and Adams in the case of Neptune. In fact Lowell himself remarked, " Analytics thought to promise the precision of a rifle and finds that it must rely upon the promiscuity of a shot-gun after all." Nevertheless, with the indefinite data available, Lowell carried out the investigation and predicted the weight of the hypothetical planet and the orbit in which it was moving. Fifteen years later, after a laborious search carried out at the Lowell Observatory in the neighbourhood of the predicted positions, a planet was found. The method used was that of the " blink microscope " which we have described in connection with the discovery of new variable stars. Two photographs are taken of the same small part of the sky separated by an interval of a few days. The two photographs are then compared minutely in the blink microscope in the hope of detecting the planet by its slight movement between the two exposures. The discovery was made by Mr. Tombaugh in January 1930, and, after its confirmation, was announced very appropriately on the anniversary of Lowell's birthday.

Pluto turned out to be a very much smaller body than that which Lowell had predicted. The weight of Pluto is probably less than one-ninth of the predicted weight. If the problem is reversed and the irregularities in the motion of Uranus are calculated from the known position and known weight of Pluto, the irregularities come out to be so small as to be practically undetectable by observation. Most authorities have therefore concluded that the surprisingly close agreement between the predicted orbit and the orbit actually found was largely a matter of luck.

When one realizes that there are about twenty million stars in the sky as bright as Pluto, and that the predicted position in the heavens was only very roughly indicated, one is filled with admiration for those who conducted the search and succeeded in it. No needle in a haystack was ever more securely concealed. The search, however, was inspired by the theoretical investigation which went before. And even if it is conceded that the successful prediction was largely a matter of chance, it has got to be remembered that, but for Lowell's investigation, the search would not have been made.

Direct telescopic observation gives us comparatively little information about these three planets. The discs of Uranus and Neptune can be seen with quite small instruments, but no telescope has yet revealed the disc of Pluto. Uranus and Neptune are considerably smaller than Jupiter and Saturn, but very much bigger than the four terrestrial planets. Pluto, on the other hand, must be considerably smaller than the earth. No definite markings can be made out on Neptune; but faint belts, similar to those on Saturn, can be seen on Uranus. By the spectroscopic method Lowell in 1911 deduced for Uranus a rotation period of ten and three-quarter hours. This result was strikingly confirmed by Mr. Leon Campbell at Harvard in 1917, who found that the brightness of the planet fluctuated regularly in a period of ten hours forty-nine minutes.

In 1883 and again in 1884 Maxwell Hall in Jamaica had noticed that Neptune varied in brightness in a period of slightly less than eight hours; but the fluctuations seemed to die out and their interpretation as a manifestation of the planet's rotation was not generally accepted. In 1922 and 1923, however, a careful study was made of the brightness of Neptune at the Tartu Observatory by Drs. Öpik and Livländer; and they also found a regularly periodic fluctuation of seven hours fifty minutes. But from various theoretical considerations, based on the observed changes in the orbit of Neptune's satellite, it was shown that so short a period of rotation was out of the question. The apparent contradiction was explained in 1928 by Drs. Moore and Menzel of Lick, who succeeded in determining the rotation period of the planet spectroscopically in spite of the minute size of the planet's disc. They found the period to be fifteen hours forty minutes, exactly twice as long as the period of light variation; so presumably there were two parts of the planet's surface brighter than the rest of it and situated more or less on opposite sides of the planet. Subsequently Dr. Jackson, now His Majesty's Astronomer at the Cape, showed that the period found by the spectroscope was closely that to be expected from the theoretical considerations we have mentioned.

Apart from the results of the spectroscopic study of their atmospheres, which we shall now consider, we thus know comparatively little about the physical condition of Uranus and Neptune. And as for Pluto we know practically nothing.

CHAPTER XXVI

PLANETARY ATMOSPHERES

As the result of many years' work at the Lowell Observatory Dr. Slipher has discovered numerous new bands in the spectra of the four great planets, especially in the red and infra-red, and has shown that as regards the positions of the bands the four spectra are the same. The increasing strength of some of these bands in passing from Jupiter to Uranus had been noticed by the earlier observers. Dr. Slipher showed that this was a feature of practically all the bands and was still more noticeable in passing from Uranus to Neptune. It was not, however, until 1932 that the mystery surrounding the origin of these bands began to be cleared up. In that year Dr. Wildt of Göttingen examined in the laboratory the absorption spectra produced by ammonia and methane and suggested that it was very probable that one of the bands in the planetary spectra was due to ammonia while four others were due to methane. In order to confirm this Dr. Dunham at Mount Wilson straightway proceeded to photograph the spectra of Jupiter and Saturn on a much larger scale than had been done before and to compare these spectra with spectra of ammonia and methane photographed on a similar scale in the laboratory. The method was to photograph the spectrum of a suitable light which was placed at the far end of a very long tube containing the gas in question. As Dr. Wildt had found, the laboratory spectra showed bands identical with those in the planetary spectra. Furthermore, owing to the much larger scale of the photographs, the bands were now seen to be made up of large numbers of fine lines; and this " fine structure " was found down to the smallest detail to be identical in the planetary and laboratory spectra.

Subsequently it was shown by Dr. Slipher, in collaboration with Dr. Adel, that whereas the methane bands increase rapidly in strength in passing outwards from Jupiter to Neptune, the bands due to ammonia are strong in Jupiter, weak in Saturn and absent in Uranus and Neptune. They also compared the spectra of all four planets with the absorption spectrum produced by a tube of methane forty-five metres long under a pressure of forty atmospheres—corresponding to a tube over a mile long at ordinary atmospheric pressure. They concluded from this comparison that the absorption by methane in the various planetary atmospheres corresponded to that produced by a tube of methane (under atmospheric pressure) half a mile long for Jupiter, slightly more than half a mile long for Saturn, four miles long for Uranus and twenty-five miles long for Neptune.

Dr. Dunham points out that the fading out of the ammonia bands in the more distant planets is a simple consequence of the comparative ease with which ammonia condenses. In the atmosphere of Jupiter large amounts of ammonia will be still in the gasous state; in Saturn's atmosphere, with its lower temperature, more of the ammonia will have condensed into clouds and less vapour will be left over to produce absorption; finally, at the still lower temperatures prevailing in the atmospheres of Uranus and Neptune the condensation into clouds will be still more complete and there will be no vapour left to produce absorption. Professor Russell has suggested that the opposite phenomenon observed for methane—its absorption *increasing* in the more distant planets—is a direct consequence of this same process. With diminishing temperature there would be an increasing precipitation of the ammonia clouds and a consequent clearing of the atmosphere above them. The sunlight would then be able to penetrate to a greater depth into the planet's atmosphere and would thus suffer absorption by a thicker layer of the methane. Hence the increasing strength of the methane bands in passing from Jupiter to Neptune is a result of the increasingly transparent atmospheres and of the increasing depths to which our spectroscopes can penetrate.

No one would deny that it is interesting to know that the atmospheres of the four large planets contain methane and ammonia, that the atmosphere of Mars contains little or no oxygen, that the atmosphere of Venus is largely composed of carbon-dioxide, and that the atmosphere of Mercury is excessively rarefied. But it might well be objected that such facts are only of local interest and have little or no bearing on the wider problems of astronomy and physics. For after all the planets are the only bodies of their kind we know of in the universe, and the interest we have in them is entirely due to the accident of their being so close at hand. It may be interesting to know the intimate details about our brothers and sisters in the solar system, but it does not necessarily follow that such details will add greatly to the sum total of our knowledge.

At the end of 1934 Professor Russell pointed out that the information which had been collected about the planetary atmospheres was very far from being a mass of " isolated facts." He showed that, so far as the observations went, these atmospheres were in composition exactly what they should be according to general physical and chemical principles; that the composition of a planet's atmosphere was determined within definite and quite narrow limits by these principles; and that presumably if any other star were to produce a family of planets, their atmospheres would be similar in composition to those of our own planets.

The spectroscope shows that the stars are probably all very similar to the sun in chemical composition. It also shows that, as far as the outer layers of the sun and stars are concerned, the most abundant element is hydrogen; while probably next in abundance are the elements helium, oxygen, nitrogen, carbon, silicon and the metals. Since it is extremely probable that the planets originated in some way or other from the sun, we may conclude that at the time of their birth they also consisted mainly of these elements. We shall now consider the course of development of an atmosphere, as given by Professor Russell's theory, first in the smaller planets, minor planets and satellites, secondly in the planets of intermediate size, and thirdly in the large planets.

A body of small size and mass would exert such a weak gravitational pull that it would be quite unable to retain any kind of atmosphere. For the molecules of a gas are in rapid motion, especially when the temperature is high, and are bound to escape gradually into space unless restrained by a considerable gravitational attraction. It can be shown that the majority of minor planets and satellites could never have had an atmosphere, having lost it, so to speak, in the process of being born. Some of the larger satellites like our moon would have retained their atmospheres longer, and may not have lost them completely until after they had solidified. The planet Mercury, which has practically the same size as the two largest satellites of Jupiter and Neptune's satellite, has according to M. Antoniadi an extremely rarefied atmosphere. Presumably therefore in the case of these heavier bodies the atmospheres may not yet have completely escaped.

The rate at which the atmosphere of a planet tends to be lost depends not only on the weight of the planet but also on the speed with which the molecules in the atmosphere are moving. The speed increases as the temperature rises, and is greater for light molecules than for heavy molecules. It follows that the warmer a planet is the more rapidly does its atmosphere tend to escape; and that light gases like hydrogen and helium tend to disappear more rapidly than heavier gases like oxygen and nitrogen.

We come now to the planetary bodies of intermediate size: the Earth, Venus and Mars. While they were still in a molten condition the hydrogen, other than that in chemical combination, and helium would have escaped completely; but the heavier oxygen and nitrogen, escaping more slowly, would remain in moderate amounts. Actually the escape of the oxygen would be hindered by another process; for, except during the initial stage of very high temperature, much of it would form oxides with the silicon and the metals and go to form part of the molten core. But whereas any nitrogen remaining in the atmosphere would exist as free nitrogen, any oxygen remaining in the atmosphere would be combined with carbon as carbon-dioxide. Thus by the time the planet begins to solidify its atmosphere will consist of nitrogen and some carbon-dioxide; and though these gases continue to escape, the rate at which they do so diminishes as the temperature falls. But now a new process sets in, for with advancing solidification large volumes of gas are evolved from the semi-molten mass. The liberated gas will consist of water vapour and carbon-dioxide—the oxygen and hydrogen contained therein having been prevented from escaping in the hotter stages by their combination with other elements. The planet is thus provided with a new atmosphere of water vapour and carbon-dioxide, together with the carbon dioxide and nitrogen left over from the primitive atmosphere. When the surface is at last well below the boiling point, the water vapour will condense out of the atmosphere and form, if present in sufficient quantity, oceans and lakes. It is important to notice that there can be no oxygen in the atmosphere of such a planet, for all the oxygen must have entered into chemical combination with other elements. If oxygen is to make its appearance subsequently, it can, almost certainly, only do so after the surface has cooled down and vegetation has started to grow upon it. For, as we have seen vegetation is able to take up carbon-dioxide from an atmosphere and, after retaining the carbon, return back to it the free oxygen—and this, so far as is known, is the only mechanism by which an atmosphere of free oxygen can come into existence.

It is probable that when life first dawned upon the earth, in the form of vegetation, the earth's atmosphere was mainly composed of nitrogen and carbon-dioxide. In the course of ages the latter gas was gradually replaced by free oxygen. The small percentage of carbon-dioxide that is now present in the air is due to the respiration of animal life—whereby oxygen is breathed in and carbon-dioxide breathed out—and to the decay of both animal and vegetable matter. Between these two processes on the one hand and the production of free oxygen by vegetation on the other, there is a steady equilibrium, which keeps the composition of our atmosphere, as far as we can tell, constant.

It is possible that Venus represents a world in a slightly earlier stage than our own; for we saw that its atmosphere contained much carbon-dioxide, yet no detectable amounts of oxygen. Perhaps for some reason vegetation has never made its appearance on the surface of that planet; or perhaps it has done so only recently. Dr. Wildt believes that owing to its nearness to the sun and to the powerful " blanketing " effect of its atmosphere, Venus may have a temperature at its actual surface close to boiling point —which would of course imply the absence of vegetation.

On Mars we are practically certain there is vegetation; on the other hand we know that oxygen is present, if at all, in only very small amounts. There are various ways of accounting for a failure of the vegetation to sustain a considerable oxygen atmosphere; but one of them has the great advantage of explaining, at the same time, the mysterious reddish colour of Mars' surface. Apart from the processes of decay and of animal respiration, there is another process which is continually removing oxygen from the atmosphere. During the " weathering " of rocks, the iron present in them in the partially oxidized state becomes completely oxidized by taking up oxygen from the air. Now this process, unlike the other two, cannot be undone by vegetation; instead it proceeds inexorably to its conclusion. And since it is believed that there is enough of this partially oxidized iron in the rocks to take up all the oxygen in our atmosphere, the conclusion will only be reached when all the atmospheric oxygen is used up. Now the fully oxidized iron compounds—the so-called ferric compounds—are of red colour. And so we may expect in the far off future, as the oxygen leaves the atmosphere and takes to the soil, that the surface of our planet will gradually become coated with a red layer of rust. This, Professor Russell suggests, is what has happened on Mars: it is a world which is approaching the end of its life-bearing period.

We come now to the atmospheres of the four great planets. Owing to their large mass they would be able to retain by their gravitational pull the *whole* of the atmospheres with which they were originally endowed by the sun. In particular they would retain the whole of the hydrogen, which as we said is probably the most abundant element in the sun and stars. As the body of the planet begins to solidify much of the oxygen is taken up in the form of solid or liquid oxides as in the case of the smaller planets; but, since none of the oxygen escapes into space, there is still a great deal left over which remains in the atmosphere. This atmospheric oxygen will be in the form of water vapour (combined with hydrogen), and of carbon-dioxide (combined with carbon). Free nitrogen also will be present in large amounts, as well as helium.

When the molten planet is still very hot, say about 1,000 degrees centigrade, experiment shows that the elementary chemical constituents would exist in the following forms: the nitrogen as free nitrogen, the oxygen in combination as water vapour or carbon-dioxide, and the excess of hydrogen as free hydrogen—the helium will also be " free " because it is an " inert gas " and is incapable of entering into any chemical union.

With falling temperature it can be shown that the elementary constituents will gradually rearrange themselves. The carbon-dioxide will interact with the free hydrogen to form methane (CH_4) and more water vapour, and the nitrogen will combine with the free hydrogen to form ammonia (NH_3). By the time the temperature has fallen to approximately the temperature of the earth, the atmosphere will consist almost entirely of the excess of free hydrogen (for hydrogen, as we saw, was probably much more abundant than any of the other elements), methane, ammonia, and the inert helium. Water vapour will also be present to some extent, but most of it will by now have condensed to form a deep ocean covering the rocky surface of the planet.

In the present condition of the major planets, with temperatures more than 100 degrees centigrade below freezing, the ocean must be frozen solid forming a great shell of ice covering the rocky core. Further the ammonia vapour must largely have condensed out of the atmosphere to form clouds; and though in the warmer planets, Jupiter and Saturn, some ammonia vapour still remains above the clouds, there is practically no ammonia vapour left in the atmospheres of the colder planets, Uranus and Neptune.

We may conclude that the atmosphere of a large cool planet will necessarily consist mainly of hydrogen, helium, methane and ammonia, together with traces of water vapour from the underlying oceans (unless the temperature is so low that these are frozen). But there will be no oxygen or carbon-dioxide. On the other hand smaller planets must have atmospheres consisting of carbon-dioxide and more or less nitrogen, provided they are large enough to have prevented all the nitrogen escaping while they were still hot. But again there will be no oxygen, though it may subsequently replace the carbon-dioxide if vegetation is able to grow. Finally, the smallest bodies, satellites and minor planets, will be unable to retain any permanent atmosphere. We repeat that since the stars all appear to be very similar to the sun in chemical composition, any planets that they may produce would doubtless approximate closely, as regards their atmospheres, to one or other of these three definite patterns. It will be noticed that only one of these three patterns can possibly become compatible with the existence of life.

CHAPTER XXVII

MINOR PLANETS AND SATELLITES

Minor Planets. The number of minor planets discovered had already exceeded 200 by the close of last century. Largely as the result of the improvements in photographic methods and of the arrangement for co-operation among observers for systematic search, the rate of new discoveries has since increased rapidly. By the end of 1936 the number of minor planets known, and for which definite orbits had been determined, was 1,380. Several hundred more had been discovered but lost again because of insufficient observations. The problem of coping with these numerous tiny bodies—the possible identification of those newly discovered with others already known, the calculation of their orbits, and the arrangement for their future observation and surveyance—is in the hands of the Rechen-institut, under the directorship of Dr. Stracke, which co-operates with some twenty observatories and other institutions. Dr. Stracke estimates that there are about 44,000 minor planets which are within reach of discovery with our present instruments. Probably most of the brighter ones have already been found.

The minor planets lie in the zone between the orbits of Jupiter and Mars, the diameters of their orbits being intermediate between the diameters of these two planets. One exception is the planet Eros with an orbital diameter less than that of Mars, and others are Hidalgo and Hector with orbital diameters somewhat greater than that of Jupiter. But the orbits are often very highly elongated and in such cases they often overlap the nearly circular orbits of the larger planets. Thus Hidalgo at its maximum distance from the sun travels almost out to the orbit of Saturn, while Amor, discovered in 1932, comes, when nearest the sun, inside the orbit of Venus.

When a new minor planet has had its orbit determined a serial number is assigned to it and a name given to it by the discoverer. The original custom was to employ names from mythology, female names for the ordinary run of planets and masculine names for such as were unusual or of particular interest. But now that such names, especially of the female variety, are becoming exhausted the discoverer may name his planet after some famous astronomer, his pet-dog or even his favourite pudding.

A few of the minor planets are large enough to reveal when close to us a perceptible disc. By direct measurement the diameters of such bodies have been determined. A few have diameters of several hundred miles, while at the other extreme there are some that are simply chunks of rock a mile or so in diameter. We cannot of course see the discs of these minute objects and we can only estimate their sizes from their distances and brightness. The brightness of many of the minor planets undergoes a periodic variation in a few hours. This is undoubtedly the result of an uneven brightness of the surface, so that the period of variation gives us the period of rotation of the planet on its axis. In some cases the variation is so marked as to suggest that the body is quite irregular in shape and not even roughly spherical. A great deal of work is now being done on the colours of these planets and on the polarization of the light they reflect. In this way it is hoped to get some information as to the character of the surfaces of these bodies, and possibly as to the existence and properties of a diffuse absorbing medium within the solar system.

We saw how the search for minor planets had resulted in the discovery of Eros, a body which occasionally came nearer to the earth than any other except the moon. During the last five years three minor planets have been found which may approach us very much more closely even than Eros. The least possible distance of Eros is about fourteen million miles; but Amor and Apollo, both found in 1932, may approach us within distances of ten and three million miles respectively. But it is Adonis, discovered at Uccle by M. Delporte in 1936, that holds the record in this respect; for, a few days before discovery, it was only one and a half million miles away, while on rare occasions it may reduce this distance to one and a third million miles. Unfortunately all three of these planets are minute, and can only be photographed during the very short period in which they are sufficiently close to us; so for the present at least they are unlikely to be of any use in the determination of the sun's distance.

It had been long ago suggested that the family of the minor planets was produced as the result of the explosion of a single planet originally revolving between the orbits of Mars and Jupiter. Later the alternative view was put forward that, at the time at which the other planets were formed, the material, which normally might have formed the hypothetical planet, condensed instead into large numbers of small bodies. Now, it has always been hoped that the arrangement of the orbits of the numerous minor planets might some day provide us with a key to the solution of this problem, and give us some indication as to which, if either, of these two theories is correct. It can be shown that if a planet between Mars and Jupiter exploded into fragments, then however drastically the orbits of the fragments were modified by the disturbing action of Jupiter, the centres of all those orbits must remain for ever equidistant from a certain definite point on the line joining the centre of the sun with the centre of Jupiter's orbit. Professor Hirayama in Japan has shown that there are at least five groups of minor planets, containing from fifteen to forty-four of these bodies apiece, that satisfy the above conditions in a striking way. It seems possible therefore that each of these groups of planets was produced by the explosion of a larger body, while the various groups may have been produced at different times as the result of a succession of explosions in the same original planet. And so the evidence, as far as it yet goes, would appear to favour the older theory or some modification of it.

One of the most interesting problems in connexion with the minor planets is concerned with the changes continually taking place in the shapes of their orbits as a result of the disturbing action of the massive Jupiter. The motion of each minor planet is determined not only by the force acting between it and the sun but also by the force acting between it and Jupiter. And since Jupiter is so massive and periodically comparatively close to the planet, this second force is by no means negligible. The motion of each of the minor planets thus affords an example of the *problem of three bodies*, which is as yet incapable of a complete solution. Long ago, however, the French mathematician, Lagrange, drew attention to a particular case of the problem of three bodies in which he showed that the planet could revolve in a stable and unchanging orbit. The special case is that in which a minor planet is moving in the same orbit as Jupiter and at such a distance from Jupiter that the three bodies (sun, Jupiter and minor planet) form the corners of an equilateral triangle. There are two points that fulfil these requirements, one on each side of Jupiter and at a distance from him equal to one-sixth of the circumference of his orbit. There are eleven minor planets known as the *Trojan group* which lie very close to one or other of these two points and behave in accordance with Lagrange's theory. Their names are Achilles, Hector, Patroclus, Nestor, Priamus, Agamemnon, Odysseus, Aeneas, Anchises, Troilus and one discovered in 1936 as yet unnamed. Six of these lie on one side of Jupiter and five upon the other. These eleven planets thus have the remarkable property of remaining always at approximately a fixed distance from Jupiter; one group forms a vanguard and precedes him in his orbit, the other group forms a rearguard and follows him in his orbit.

Satellites. All the planets have one or more satellites with the exception of Mercury and Venus—of Pluto, of course, we know nothing, for at his immense distance no satellite of ordinary size could be seen. Just as the planets are small in comparison with the sun, so the satellites are small in comparison with their parent planets; only the moon is exceptional in this respect, for its diameter is as much as a quarter that of the earth. The diameters range from about 3,000 miles for the two largest satellites of Jupiter to about ten miles for the satellites of Mars. The majority of the satellites show no perceptible discs in the telescope; but the four larger satellites of Jupiter show discs on which markings can be actually made out; and the disc of Titan, the largest of Saturn's satellites, is just perceptible.

From the changing appearances of the surface markings on Ganymede and Callisto, Jupiter's two largest satellites, the rotation periods have been determined. In addition all four of his largest satellites as well as some of Saturn's satellites show regular variations in their brightness. Such variations must be due to unevenness in the reflective power of their surfaces, and thus give us a measure of their rotation periods. This is beautifully confirmed in the case of Ganymede and Callisto, for which the two methods give the same rotation periods. Every satellite for which the rotation period is obtainable shows the same peculiarity: the rotation period is identical with the period of orbital revolution, which means that the satellite always keeps the same side to its parent planet. This we saw was the result of the tidal action exerted by a planet on its satellite, which gradually slows down the rotation of the satellite until its period of rotation is equal to its period of revolution. Thus it has come about that the moon keeps always the same face to the earth and Mercury always the same face to the sun.

Now the effectiveness of a planet in slowing down the rotation of its satellites falls off very rapidly at increasing distances from the planet; and from this, as M. Antoniadi has recently pointed out, one can draw important conclusions as to the rotation periods of those satellites in which no variation of brightness has been detected. Thus it follows that Jupiter's "fifth satellite," which lies *nearer* to the planet than the four large satellites, must like them turn always the same side to Jupiter. Again Iapetus is the most distant of the Saturnian satellites for which the rotation period has been found by direct observation; consequently the seven satellites which lie nearer to Saturn must, like Iapetus, keep always the same face to the planet. As regards the two satellites of Mars, the four satellites of Uranus and the solitary satellite of Neptune the argument is less direct. It can easily be shown, however, that the effectiveness of these planets in slowing up the rotations of even their most distant satellites is greater than that of Saturn in slowing up the rotation of Iapetus. Therefore on the assumption that all the planets and satellites are approximately of the same age, it is most probable that like Iapetus the satellites of Mars, Uranus and Neptune have already been reduced to the state of revolving and rotating in the same period. As to Phoebe, the ninth and most distant satellite of Saturn, and the four minute outer satellites of Jupiter, Nos. 6 to 9, it is impossible to come to any definite decision.

The great majority of the satellites revolve round their orbits in the same direction as their planets rotate and in the same direction as the planets themselves revolve round the sun. There are, however, certain striking exceptions of two different kinds. First there is the case of the most distant satellite of Saturn and of the two outermost satellites of Jupiter, which traverse their orbits in the direction opposite to that in which all the other satellites of these planets move. It has been suggested that these three satellites, instead of originating in some way from their respective planets, were once minor planets which, venturing too close to the planets round which they now circulate, were so to speak captured by them. On critical examination, however, the theory turns out to be improbable. The other kind of exception to the " one-way traffic rule " of the solar system is when with one accord all the satellites of a planet as well as the planet itself transgress against it. This happens in the systems of Uranus and Neptune. Thus Uranus is found to be lying, so to speak, on its side with the plane of its equator almost perpendicular to that of its orbit, while the orbits of the satellites are tipped up to the same steep angle. Actually this angle is just less than a right-angle and the direction of rotation of the planet and of revolution of the satellites is opposite to the normal direction in the solar system. The angle at which the equator of Neptune and the orbit of its satellite is tilted to Neptune's orbit is much smaller; but again the rotation of the planet and the revolution of the satellite are in the backward direction. It has been suggested that originally the systems of all four major planets spun in this backward direction, and that by some process, which acts fastest for the systems nearest the sun, they are turning or have turned turtle. We should then look upon these two systems *not* as spinning backwards, but as spinning forwards *upside-down*; and regard the angles at which they are tilted to the planetary orbits as *bigger than right-angles*. We then find a very striking progression in these angles as we pass from one planet to the next: 151 degrees for Neptune, 98 for Uranus, 27 for Saturn, and 3 for Jupiter. Professor Stratton has shown that under certain circumstances the combined tidal effects of the sun and satellites might be responsible for a process of this kind. But again it is very questionable whether the explanation is correct.

But perhaps the most interesting of all the satellites is Phobos, the inner of Mars' two satellites. What is more, the theory put forward to account for its remarkable behaviour will not have to be abandoned as too improbable in the next sentence! It seems that Phobos affords the unique example of the process which ultimately leads to a structure of the type of Saturn's rings. It is the only satellite in the solar system that revolves round its parent planet in a shorter period than that in which the planet rotates on its axis. Mars rotates in a period slightly longer than twenty-four-and-a-half hours yet during that time Phobos revolves round the planet about three times; thus on Mars the day is three times as long as the month, though of course since there are two moons there will be two kinds of " month."

We saw that the tides which the moon exerts on the earth have a braking effect on the earth's rotation, slowing it down and increasing the length of the day; and that consequently, in accordance with the principle of the conservation of angular momentum, the moon must be slowly receding from the earth. Further we saw that the recession of the moon and the slowing down of the earth's rotation would continue until the earth kept always the same face to the moon, when the day and the month would be equal, when the tides would no longer ebb and flow, and when in fact their braking action would be at an end.

Now, if it were simply a question of the earth and moon alone, this would be the end of the matter. But there are still the solar tides; and these continue to slow down the rotation of the earth just as they have, with demonstrable success, slowed down the rotation of Mercury. Thus the day grows still longer and becomes for the first time even longer than the month. So once more the lunar tides begin to ebb and flow; but now, since the moon revolves in a *shorter* period than the earth rotates, their effect is the opposite to what it was before and they tend to impede the further lengthening of the day. According to the theory of tidal evolution the result of these opposing actions of the sun and moon upon the earth's rotation will be to make the moon, which had already stopped receding from the earth, start approaching it again. Very slowly then, the moon will come nearer and nearer while all the time the speed with which it is revolving will increase. This is exactly the state of affairs exemplified by Phobos: the day being longer than the month, and the month itself getting shorter and shorter. The final act of the drama ends unexpectedly and the apparently inevitable tragedy is averted: for the moon instead of crashing to the earth would on coming within about 6,000 miles of its surface burst asunder into countless fragments. The reason for this break-up was, we saw, pointed out by Roche in 1848. He showed that, within a distance of about two-and-a-half times the radius of a planet from its centre, the tidal forces exerted by the planet would break any satellite into pieces. Phobos is already within less than 1,000 miles of Roche's limit for Mars, but we have not the least idea how long it will be before it enters the danger zone. It is significant that the rings of Saturn lie well within Roche's limit for this planet. It is in fact highly probable that the rings are composed of the shattered fragments of a large satellite that approached too near its parent; for the fragments would presently become spread out evenly round the planet in just such a flat ring as Saturn has. As far as our moon is concerned, this final metamorphosis must be incredibly remote. Many thousand million years must elapse before the moon even starts upon its return journey to the earth. Perhaps the sun will have already ceased to shine; and the terrestrial ring, born into the blackness of eternal night, would remain as a spectacle for ever unrecorded.

As regards the surface and physical condition of our own satellite we need say but little, for the earlier observations already discussed have in general been confirmed. Modern photographs, especially the very fine ones taken in recent years with the 100-inch telescope, have proved of great service in the accurate mapping of the moon, and in providing impersonal and permanent records even of the finer details with which in the future comparisons can always be made. But as in all large-scale astronomical photography the detail revealed is never so delicate as that which can be seen with the telescope. So visual observations and drawings are still of importance, though they are necessarily confined to the study of small regions of special interest. So far no changes have ever been detected that could not be accounted for by the variations in the angle of the illumination in which the landscape is observed. The absence of any atmosphere, deduced long ago from the suddenness with which stars disappeared behind the moon, has been fully confirmed.

In the last few years M. Lyot has made an extensive polariscopic survey of the surface measuring the degree of polarization of the light from the various surface features—the mountains, the plains, and the brighter and the duller areas—under different angles of illumination. He finds that his results are in accordance with the view that the surface has the nature of volcanic ash and pumice. This conclusion, it appears, would fit in well with the rapid fall in temperature measured by Drs. Pettit and Nicholson at Mount Wilson during total lunar eclipses: for when the " mid-day " sun of our satellite is suddenly cut off, the temperature of the moon's surface drops from 120 degrees centigrade above, to 100 degrees centigrade below freezing-point in the course of one hour.

We can have little doubt that the mountain chains, peaks and valleys on the moon were formed in the same way as similar structures were formed upon the earth. But astronomers have had great difficulty in arriving at an explanation of the so-called " lunar craters." The craters are walled-plains ranging from over 100 miles in diameter to objects that can be barely seen in the largest telescopes. The walls are generally practically circular and often tower precipitously to great heights, some reaching over 20,000 feet. The floor of the crater is below the level of the surrounding surface unless the crater happens to be situated already on high-lying ground. In the larger craters there is often a central mountain peak or group of peaks; but these are not found in the smaller craters.

The craters used to be thought to be volcanic in origin—hence their name. But though this theory has gained wide acceptance there is no evidence whatsoever in support of it. No terrestrial volcano resembles the lunar craters in form or approaches even the smaller of them in size. Another theory supposes that the craters represent bubbles which were blown by escaping gases in the lunar surface while it was still molten; and which, in the process of being blown, burst. But it is certain that no bubble of such size could possibly be blown. The only theory for which the evidence is at all satisfactory supposes that the craters were produced by a meteoric bombardment. As originally put forward by Proctor towards the end of last century, it suggested that the bombardment occurred while the surface of the moon was still plastic, for otherwise it was difficult to see how it was that the craters were all practically circular.

But in 1915 A. W. Bickerton showed that if the bombardment had occurred after the moon's surface had solidified, several difficulties could be immediately explained. Now a meteorite will hit the moon's surface with its full velocity of a great many miles a second, for there is no lunar atmosphere to slow it down. Bickerton showed that a solid body hitting the solid surface of the moon with so great a velocity would act as if it were a high-explosive shell—only much more so. It would throw up around it an encircling wall of dimensions altogether out of proportion to its own size; and this wall would invariably be circular, irrespective of the angle at which the meteorite hit the moon's surface. Thus the great size of the craters and their practical circularity—the two points most frequently urged against the meteoric theory of their origin—are actually the points in which this theory is so much superior to other theories. A possible explanation as to why the moon suffered and the earth apparently escaped is also forthcoming: the earth may have been largely protected by its atmosphere; or its surface may at that time have still been plastic so that any " shell-holes " which were made subsequently closed over.[1] In support of this theory we shall see later on that there are on the surface of the earth a few small craters very similar in *form* to the lunar craters, which have undoubtedly been formed by meteorites. During the great war the shell-holes made by high explosive-shells also resembled in structure the craters on the moon.

The motion of the moon under the gravitational influence of both earth and sun is the classical example of the problem of three bodies. As such it is incapable of a complete solution. But even the approximate prediction of the moon's position in the heavens is greatly complicated because of the superposed influences of the various planets. The different effects, or *terms* as they are called, produced in the moon's motion in different ways by the different bodies, are very numerous; and the business of allowing for each one of them in the prediction of the moon's position is not only extremely complicated but extremely arduous. The most recent and exhaustive investigation of the " lunar problem " is that of Professor E. W. Brown of Yale who has devoted over thirty years to the work. His tables of the moon take into account no less than 1,500 separate *terms*, thus enabling the positions of the moon to be calculated without the terrific labour of having to recompute these 1,500 terms for each calculation. In 1923 these new tables replaced the tables of Hansen, which since 1857 had been used for the computations in the *Nautical Almanac*.

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[1] Dr. L. J. Spencer, the geologist, has recently pointed out that the effects of atmospheric " weathering " on the Earth would in any case have sufficed to obliterate even the largest craters thus formed in remote ages.

It has long been known that there are irregularities in addition to those due to the above causes and which gravitational theory alone fails to explain. These consist in the moon getting ahead of, or lagging behind, the scheduled position in its orbit, the tendency in the long-run being to get ahead. It has been long known that such irregularities in the " speed " of the moon *might* be apparent rather than real, the fault, so to speak, being in our clocks rather than in the moon. But only during the last fifteen years has evidence been forthcoming that this is actually the case. The time-keeping of our clocks is entirely dependent on astronomical observation, and thus on the regularity of the earth's rotation. If the period of the earth's rotation varied at all, our clocks would follow suit, and we should have no means of detecting the variation in our time-reckoning. The only observable effect would be that a body like the moon—whose revolution in its orbit is quite independent of our time-standard, the period of the earth's rotation—would sometimes appear to be ahead of, and sometimes behind its predicted place. Even so one would not be able to tell whether it was the earth's rotation or the moon's revolution that was irregular. The only way would be to examine the motions of other revolving bodies; because if it was the earth's rotation that was at fault, such bodies would show in their motions irregularities similar to, and *synchronous* with, those in the moon's motion. Unfortunately the expected irregularities would be quite undetectable in the observations of the majority of planets and satellites; and practically the only bodies suitable for the purpose—which in fact is to make use of them as independent clocks—are the earth and Mercury in their orbital motions round the sun, and the innermost of the four large satellites of Jupiter in its orbital motion round Jupiter. Some years ago Innes, the Director of the Union Observatory at Johannesburg, obtained evidence which suggested that these bodies did copy the tendency of the moon to gain in the long-run on the predicted motion. Later, in 1926, Professor E. W. Brown found that the irregular speeding up and slowing down of the moon, superposed on its general speeding-up, was also copied by these bodies. Thus it seems likely that part at least of the unexplained irregularities in the moon's motion is due to a variation in the earth's rotation. Probably a portion of this variation consists in a progressive increase in the length of the day, due to the previously discussed tidal effect of the moon. Professor

Brown believes that the remaining irregular portion is also produced by variations in the length of the day; and these he suggests are due to alternating expansions and contractions of the earth's crust amounting to a few feet.

One may ask what possibilities there are of some day making a clock which keeps such perfect time as to afford an independent check on the regularity of the earth's rotation. The most perfect clock available so far is the Free Pendulum Shortt clock, the first one having been installed at Greenwich in 1925. The Royal Observatory now possesses four of these clocks and is about to instal a fifth; and altogether there are about sixty of them distributed among the observatories of the world. Though their accuracy far surpasses that of any other clock, they are not perfect. According to the Greenwich experience, as given in the Astronomer's Royal report for 1937, they may be subject to erratic changes of rate of one or two hundredths of a second per day; while, even with three such clocks running for inter-comparison, there may accumulate by the end of a long cloudy spell, say a fortnight, a doubt as to the correct time of as much as five hundredths of a second. For some years experiments have been in progress to try out methods of time-keeping which depend on various electrical instead of mechanical principles, one of which is the use of the crystal oscillator. About 1928 Mr. Alfred Loomis of Tuxedo Park, N.Y., made elaborate comparisons between a crystal oscillator clock and his three Shortt clocks, and found that whereas the crystal clock was the more accurate over periods of hours, the mechanical clocks were more reliable over longer periods of time. A crystal oscillator clock is now to be installed at Greenwich.

The amount by which the moon gets ahead of its predicted position, after all allowance has been made for the gravitational effects —the amount therefore which is presumably due to the lengthening of our day by the tides—can be determined from the study of ancient eclipse records. The most important recent work on this subject has been done by Dr. P. H. Cowell, a previous Superintendent of the Nautical Almanac Office, and J. K. Fotheringham of Oxford, during the last thirty years. The results, which are of course liable to some uncertainty, show that the moon gets ahead by several hours in a few thousand years; but this is large enough to emerge fairly definitely in many of the ancient records. The result which has been adopted by Professor Russell as the most probable one is that the length of our day is increasing at the rate of one thousandth of a second per century. This would mean that looked upon as a clock the earth would *lose* rather more than two *hours* in 2,000 years, and rather less than one two-thousandth of a *second* in one year. Clearly, therefore, the Shortt clock and the crystal oscillator have a very long way to go towards improving their accuracy before they can be used as a direct check on this *progressive* lengthening of our day. On the other hand the irregular oscillations that are superposed on the progressive advance of the moon in its orbit are very much larger; and these, as we have seen, have also been ascribed by Professor E. W. Brown to changes in the length of the day—to sudden changes due to expansions and contractions of the earth's crust. Now these sudden changes may affect the rotation of the earth in such a way that regarded as a clock the earth may *gain* or *lose* as much as twenty seconds in ten years; and this " error " is just about comparable with that of our best clocks. Thus there is every hope that within the next few years we may obtain from our clocks a direct check on the grosser irregularities in the length of the day.

CHAPTER XXVIII

COMETS

THOUGH in general the study of these bodies has advanced rapidly of late years, it has in certain respects remained practically stationary. In particular we are still largely ignorant of the processes responsible for the physical development of a comet as it approaches the sun: the production of the tail and the mechanism by which both head and tail may emit light of great brilliance. One reason has been the extraordinary scarcity of bright comets during the present century. The latter part of last century was peculiarly rich in them; but then the spectroscope and the photographic plate were still in their infancy. In 1910 there were two magnificent comets; but since then there has been but one really bright comet, and that remained visible for only a few days and was only to be observed close up to the sun in broad daylight. Since the most important physical phenomena are displayed but feebly, if at all, by the lesser comets, it follows that there has been little or no opportunity for studying them with the most modern spectroscopic methods. Undoubtedly a great deal will be learned when the next " hairy visitor " stretches its tail across the heavens.

Mainly through the use of photography the number of faint comets discovered has been much increased during the last twenty or thirty years. The average yearly yield is five or six, and 1932 holds the record with thirteen. Only a small percentage of these attain naked-eye visibility, and of those that do very few would be noticed unless one knew exactly where to look. A comet generally begins to develop a tail about the time that it reaches naked-eye visibility; so the majority of comets are tail-less objects, and in the telescope are only to be distinguished from a nebula by their motion among the stars.

The close connexion between comets and meteor swarms, discovered by Schiaparelli, has been fully confirmed. In five cases the identity of the orbit of a meteor swarm with that of a comet has been established beyond doubt, and in several other cases the identity is strongly suggested. We may therefore conclude that a comet is actually a large and comparatively compact swarm of meteors. In the course of ages, as the result of its continual journeying round the sun, the swarm tends to become dispersed so that its component particles gradually become strewn more evenly round the entire orbit. This scattering of the swarm is mainly brought about by the tidal action of the sun which is strongest when the swarm is at its nearest to that body; it thus proceeds more rapidly in those swarms (or comets) that pay frequent visits to the sun's vicinity, in those, in fact, which have small orbits and revolve round him in short periods. Moreover the longer a swarm has been in existence the further will its ultimate dissipation have proceeded. In a very ancient swarm we might expect the comet itself to have become so impoverished as to be very faint or invisible; while on the other hand we might expect the train of particles strewn behind to have become so evenly distributed round the orbit that the annual shooting star showers they give rise to would differ but little in splendour from year to year. From this we may infer, for example, that the August shooting stars, which display a very constant degree of activity, come from a much more ancient swarm than the shooting stars of the November shower, which though usually a light " drizzle " tends at intervals of thirty-three years, or multiples thereof, to be a regular downpour.

It is important to realize that comparatively few comets can give rise to shooting stars. Only those can do so whose orbits actually cut through the earth's orbit. The orbits of comets tend to be steeply inclined to the plane in which the orbits of the earth and the eight other planets lie; so the majority of these bodies never pass anywhere near the earth. When the orbit of a comet does cut through the earth's orbit, then at each of its journeys round the sun the earth must cross the comet's orbit and plough its way through any meteoric particles that happen to be passing at the time. We shall thus get a shower of shooting stars from the comet every year on the same date. It must also be clearly understood that the trail of meteoric particles strewn along the comet's orbit has nothing whatever to do with the comet's tail. The tail, which we shall discuss in a moment, is a *visible* appendage which extends from the head almost directly away from the sun, and so does not in general stretch out along the comet's path. The trail of meteoric matter in the orbit, on the other hand, is quite *invisible*, because the particles forming it are so diffusely scattered. The only occasions on which we become aware of its existence are those on which the earth ploughs through it while crossing the comet's orbit. Some of the particles then crash into the earth's atmosphere and, owing to the heat engendered by their rush through it, are momentarily raised to a brilliant incandescence. Thus the particles which compose the meteoric trail of a comet only become visible to us in the moment of their death agony; most often they are completely burnt out in the upper atmosphere, but a few survive long enough to reach the ground.

Donati in 1864 was the first to examine a comet with the spectroscope. The light of the comet, which was only of middle rank, was seen to consist of three bright bands—blue, green and yellow—separated by dark intervals. The light was thus largely emitted by the comet itself and not simply reflected by it from the sun. Another somewhat feeble comet was examined spectroscopically by Huggins in 1868. He showed that the bright bands in the spectrum of the comet were identical with those emitted by certain hydrocarbons when excited in a vacuum tube by an electric discharge. The first photographs of a comet's spectrum were obtained by Huggins, and about the same time by Draper in America, in 1881. They fully confirmed the origin of the bright bands from compounds of hydrogen and carbon, but they showed also the presence of a continuous spectrum crossed by dark lines, a replica of the solar spectrum. This was the first indication that, in addition to emitting light of its own, a comet shone by reflecting sunlight. The next important piece of spectroscopic information was derived from the great comet of 1882 which was discovered at the Dudley Observatory, Albany, and was at one time readily visible to the naked eye within a hand's breadth of the noon-day sun. While the comet was still at some distance from the sun its spectrum showed the ordinary hydrocarbon bands with the dark-line solar spectrum as a background. But as it got closer to the sun both bright and dark lines became drowned by the brilliance of the continuous spectrum; and later, as it approached still nearer, the continuous spectrum was crossed by two almost solitary and dazzling yellow lines, the rays of sodium vapour.

These broad facts, gleaned during the first years of spectroscopic study, have been substantially confirmed. It is found that practically all comets give the typical bands due to compounds of carbon and hydrogen, though others due to compounds of carbon with oxygen and of carbon with nitrogen (carbon-monoxide and cyanogen) are also generally present. The proportion of the *emitted* light to the *reflected* light turns out to be very variable, both from one comet to the next and at different stages in the same comet. In general the proportion of the light that is emitted increases rapidly as the sun is approached. Finally, it appears to be the normal rule, when a large comet gets very near the sun, for the bands of the carbon compounds to fade out and be replaced by the bright lines of sodium and even other metallic vapours such as iron. On occasions striking differences have been noted in the spectra of the head and tail. But only rarely have comets' tails been bright enough for their spectra to be photographed; and we shall have to wait for the next really great comet before it is possible to study these differences in detail.

As a comet approaches the sun and earth it brightens up very much more quickly than can be accounted for by its diminishing distance from the two bodies. It is found that this abnormal rate of brightening is mainly dependent on the diminution in the distance between the comet and the *sun*. There is no doubt that this is due to some influence exerted by the sun which causes the comet to emit, as it gets nearer the sun, radiations of rapidly increasing intensity. The spectroscopic phenomena just mentioned point strongly to the same conclusion. But some comets brighten up much more rapidly than others; while a given comet may undergo sudden and unexpected variations, increasing or diminishing its brightness several-fold in the space of one night. It is clear therefore that comets vary considerably in their susceptibility to the stimulating influence of the sun, and that the susceptibility of a particular comet may alter in a quite capricious way.

Still more striking evidence of some potent influence emanating from the sun is afforded by the development of a comet's tail. The tail makes its appearance when the comet begins to get comparatively near the sun and disappears again as the comet recedes. Further, the tail is always directed away from the sun, so that it follows *behind* the comet during its approach and goes on *ahead* of the comet during its departure. It is quite evident therefore that the production and direction of the tail is connected in some fundamental way with the sun. We saw that in the original theory due to Olbers the tail was supposed to be formed by particles that were subject to repulsive forces exerted by the sun and to a less extent by the comet's head. Later Zöllner had suggested that the repulsive force was electrostatic. Developing this suggestion Bredichin had supposed that the repulsive force acting on the particles would increase in proportion as the molecular weight of the particles diminished. Hence the repulsive force would act most vigorously on hydrogen which has the lightest molecules, less vigorously on the various carbon compounds, and least vigorously on the metals with their comparatively heavy molecules. And since the most rapidly repelled particles would produce the straightest tails, he had suggested that the straight tails were composed of hydrogen, the most curved tails of metallic vapours, and the intermediate tails of the hydrocarbon gases. But Bredichin's theory has not stood the test of time; and in particular observation shows that the various gases are definitely not segregated each into its particular sector of the tail.

It is now generally held that the force exerted on the comet by the sun and responsible for the development of the tail is the light pressure, or " radiation pressure," which as we saw played such an important part in counteracting the enormous pressures inside a star. In the case of the sun, which is comparatively cool as stars go, the repulsive force exerted by its radiation is very small. Its effect on bodies of ordinary dimensions is in fact quite negligible in comparison with that of the opposing force of the sun's gravitational attraction. Nevertheless the pressure exerted by sunlight can be measured by delicate experiments in the laboratory. Now it can be shown that, if we consider particles of ever diminishing size, the gravitational force to which they are subject falls off more rapidly than the repulsive force of radiation. When we get down to dust particles with diameters of about one twenty-thousandth of an inch, the repulsive force of radiation begins actually to exceed the attractive force of gravitation. Hence particles in the neighbourhood of the sun of sizes smaller than this will, instead of falling towards the sun, be driven away from it. (The reason why the atoms in the solar atmosphere, which are of course much smaller than the smallest dust particles, have not been driven away by the same process, is that they are transparent to most of the sun's radiations and intercept only those parts of the spectrum where they produce absorption lines. Nevertheless in the case of the lightest atoms with absorption lines in the brightest parts of the solar spectrum, the gravitational attraction may be largely neutralized by the radiation pressure, so that these atoms rise to great heights in the sun's atmosphere.) It follows from the above that particles of dust present in the head of the comet, which are slightly smaller than the critical size, will be repelled feebly from the sun, while particles of dust, which are very much smaller than the critical size, will be repelled very vigorously. Moreover, the less dense a particle is the greater will be the acceleration imparted to it by the repulsive force. So we should expect the straight tails of a comet to be made up of the smallest and lightest dust particles, and the more highly curved tails to be made up of the larger and denser particles.

Although there can be but little doubt as to the general correctness of this theory, the observational evidence in support of it is still far from complete. The direct photography of comets has, however, shown definitely that the material in the tail of a comet is, as it should be, travelling outwards from the head away from the sun. Moreover it is found that this material is moving outwards with a *velocity that is continually increasing*, i.e., with an acceleration. This definitely proves that the material is moving under the influence of a *continually* acting force emanating from the direction of the sun, and not under the influence of some *momentarily* acting force such as might be supposed to arise from an explosion in the comet's head. For in this latter case the material would be driven out with a *diminishing*, instead of an increasing, velocity. This was well shown by photographs taken at short intervals of Halley's comet in 1910 and of Morehouse's comet in 1908. The theory, however, cannot by itself explain the formation of the envelopes that appear on the sunward side of the heads of certain comets; for it seems that here the material first moves *towards* the sun and subsequently sweeps backwards into the tail. It is therefore necessary to suppose that there are certain forces latent within the comet itself which may under special circumstances be set free. The envelopes could then be explained by material being explosively ejected from the comet's head: the part of the material ejected towards the sun would have its velocity gradually diminished and finally reversed by the sun's radiation pressure; and the material itself would curve back towards the tail just as the water ejected from a fountain curves back towards the ground. And there are other phenomena that require for their explanation the action of forces inherent in the comet itself. Occasionally, as in Morehouse's comet, the tail has undergone rapid distortion in shape, the further part of the tail appearing as though it had become dislocated from the rest of the tail. Phenomena of this sort suggest the action of disruptive forces of considerable violence originating in the tail. Again, it has been pointed out that the direction of the tail and the curvatures of its different parts are not always to be accounted for by radiation pressure alone; and it is suggested that there may be electrical forces at work by which the particles in the tail repel one another. But perhaps the most striking evidence for the existence within a comet of some latent force of considerable magnitude, was afforded by the behaviour of the comet Schwassmann-Wachmann discovered in 1925; for in 1932, when at a very great distance from the sun—actually eight times the distance of the earth from the sun—this comet quite unexpectedly increased its brightness within four days more than one hundred times.

As regards the mechanism by which the gaseous molecules in a comet—those of the hydrocarbons, of cyanogen and of carbon-monoxide—are made to emit light, nothing at all definite is known. Dr. Zanstra's work suggests that the molecules absorb energy from the sun's radiations and then re-emit it. Others believe that these gases are excited to luminosity in a comet in the same sort of way as they are in a vacuum tube on the passage of an electric discharge.

It is clear therefore that we are still dismally ignorant about the details of the processes going on within comets. Nevertheless a great deal of data is being laboriously accumulated particularly by M. Baldet of Meudon, by Dr. Bobrovnikoff of the Perkins Observatory, Ohio, and by many of the Russian astronomers. Our chief grounds for optimism in the future is that we are well prepared for the arrival of the next great comet; and sooner or later one is bound to turn up.

We have seen how a comet must be slowly disintegrating as in the course of ages the meteoric particles composing it tend to become strewn around the orbit. But another drain upon the comet's capital is its having to produce, at each approach to the sun, a new tail—for it is quite certain that the material which goes to form the tail is never recovered by the comet. While at a very great distance from the sun—at a distance at which it would be quite invisible in any telescope—a comet is probably a simple swarm of solid particles shining faintly in the feeble sunlight. As it approaches the sun and begins to be warmed up by its rays—and possibly stimulated by them in a manner not understood—gases and particles of dust may be expelled from the larger particles. Soon the gases begin shining by the absorption of the sun's energy, while the finer dust particles begin to be driven out by radiation pressure to form a tail. These particles in the tail may also expel gases as they are heated up by the sun. When the comet begins to retreat from the sun the solid particles in the head gradually cool down again until the further liberation, from their interstices, of gas and dust ceases. The comet now hibernates, and indulges in no further internal activity until its next approach to the sun's vicinity. Although the expenditure is probably very small compared with the total capital, one cannot help being surprised that a comet shows so little sign of wear and tear. Encke's comet has made more than forty returns since its discovery and yet it has not appreciably altered in brightness; and Halley's comet, which is famous for the length and brilliance of its tail, does not seem to have " gone off " in any way during the last three centuries. On the other hand certain of the fainter comets which return frequently have been lost ; while Biela's comet after four normal apparitions was seen to divide into two at the fifth, return as a double comet at the sixth, and thereafter was never seen again.

The head, or coma, of a comet varies enormously in size from one comet to another and from time to time in the same comet. The average size is from about 30,000 to about 150,000 miles; some are much larger, but those with diameters less than 10,000 miles are unlikely to be discovered. The *coma* of a comet—for it is usual to talk about the *head* only after a tail has appeared—generally swells as it gets nearer the sun. This is possibly due to the oozing out of gas and dust from the larger particles. When first seen at its 1910 visit Halley's comet had a diameter of 14,000 miles, yet later, when much nearer the sun, the diameter of the head rose to 200,000 and even 300,000 miles. But when actually at its nearest to the sun the diameter of the head fell temporarily to less than 60,000 miles. This temporary diminution in size when a comet is at its nearest to the sun is of common occurrence. In the middle of the coma or head of a comet there is usually a central nucleus of more intense light. This nucleus has generally a diameter of only a few hundred miles.

The length of a comet's tail is even more variable than the diameter of its head. If the tail is visible to the naked eye it is usually not less than five or ten million miles long. Many comets have had tails about fifty million miles in length, and occasionally the tails they have have been traced to a distance of nearly 100 million miles from the head.

Though the bulk of a comet is enormous, its mass is so small that it is impossible to weigh it. A heavenly body is weighed by measuring its disturbing effect on the motion of some other body which it approaches closely. Comets have passed close to the earth and close to Jupiter. During such encounters the motion of the comet has been markedly disturbed, but the most careful observations have failed to detect the slightest alteration in the motion of the planet. All that can be said, therefore, is that the mass of a comet is less than a certain amount. It is generally agreed that the largest comets cannot have more than one millionth of the earth's weight, and though, astronomically thinking, that would be a negligible figure, it would nevertheless be 6,000 million million tons.

It has been said that the whole of the vast bulk of a comet might be packed inside a Gladstone bag. The figures just given show this to be a gross exaggeration, as indeed it was doubtless meant to be; nevertheless it gives one qualitatively what is essentially the right idea. Actually if we take a comet's head of average size and assume it to have one millionth of the earth's weight, which is a very generous allowance, it turns out to be more than two hundred thousand times as rarefied as the air in a room. Though the *average* density of a comet's head must be excessively low, it is quite certain that the particles of which a comet's head is composed are comparatively heavy like the meteorites which have been found and which consist of stone or iron. But we have practically no idea how *large* these particles are likely to be. The average meteor seen in the sky is probably not much larger than a grain of sand; and the largest meteorite known, which was found in South Africa, weighs about sixty tons. So it is possible that the meteoric particles which form a comet's head are on the average intermediate between these two extremes. In any case the particles in a comet's head will have to be very widely scattered, only a few of them in every cubic mile of the comet's volume.

But though the quantitative evidence as to the density of a comet is very weak, the qualitative evidence is to say the least dramatic. As a comet traverses the sky the stars behind it shine quite undimmed even when seen through the very centre of the head. Again, the great comet of 1882 and Halley's comet in 1910 passed exactly between us and the sun, yet in neither case could any trace be found of the comet's head in silhouette against the disc of the sun. As far as we can tell therefore a comet's head has perfect transparency.

People often inquire what would be the effect of a collision with a comet. The earth must frequently have passed through the tail of a comet. It certainly passed through the tail of the comet of 1861 and probably through the edge of the tail of Halley's comet in 1910. Not only did nothing untoward happen, but no sign of the event, atmospheric or electrical, could be detected. The complete absence of any effect is exactly what one would expect; for not only must the *average* density of the tail be enormously less than that even of the head, but its component particles, unlike those of the head must *all* be excessively small. A direct collision with the head of a comet must on the other hand be extremely rare. Such a collision, it has been estimated, will occur on the average once in about fifteen million years. Oddly enough in June 1921 we only just missed colliding with Pons-Winnecke's comet, the comet having passed by a few days before we crossed its orbit. If the collision had occurred we can at least be certain that we should have had a shower of shooting stars far finer than any yet recorded. It is possible, however, that a comet's head contains a higher proportion of large meteoric particles than the swarms we commonly run into. Particles much smaller than a pea would be sufficiently affected by radiation pressure for their orbits to be gradually modified; they would thus tend to leave the main swarm of the head and to become scattered round the circuit of the comet's orbit. Further, the capacity of a comet to go on liberating, at each return to the sun, enough gas and dust for the production of a new tail, suggests that the particles containing the gas and dust must be comparatively bulky. It is possible that some of them are comparable with the larger meteorites found on the earth, which as we saw weigh up to about sixty tons. Some might be even larger—for there is no reason why a comet's head should not be exceptional in this respect. But if so, it is probable that the *majority* would have moderate weights ranging from a few ounces to a few tons (the weights found among meteorites), for otherwise it is difficult to understand the *readiness* with which the gas and dust are liberated. The effect of a collision must largely depend on whether the weights of the majority of the particles are nearer the upper or the lower of these two limits; for whereas the smaller ones would be completely vaporized in the upper atmosphere, the larger ones would reach the ground. Almost certainly some damage would be done to life and property on the side of the earth with which the collision occurred, but how much we have no idea. We shall refer to this matter again when we come to consider the great meteorite that fell, fortunately in a desolate part of Siberia, in 1908.

It is generally said that comets may move in elliptical, parabolic or hyperbolic orbits. Since elliptical orbits are *closed*, comets moving in them must return to the sun at regular intervals. On the other hand parabolic and hyperbolic orbits are *open* at their " far ends," so any comets moving in them can pay only a single visit to the sun's vicinity. An ellipse may have any degree of elongation, but sooner or later its two arms curve towards one another and join up. In a parabola the two arms tend at great distances to become parallel to one another, but they never approach one another again. The two arms of a hyperbola also tend ultimately to become straight lines, but they continue for ever to diverge from one another, though the angle of divergence may be large or small. It is clear, therefore, that the parabola forms the borderline between ellipses and hyperbolas; and that it will be impossible to distinguish a parabola from an extremely elongated ellipse on the one hand, or from a hyperbola with a small angle of divergence on the other. A parabola, in fact, represents an *ideal*, which one will never actually meet with in nature. When therefore a comet is said to travel in a parabola, one means that its orbit is indistinguishable, as far as the observations go, from an extremely elongated ellipse or from a hyperbola of small divergence.

The practical difficulty in discriminating between these three very similar types of orbit is much enhanced by certain complications that are inherent in the observation of comets. A comet is bright enough to be observed only when it is comparatively near the sun; its motion, therefore, can only be studied over an extremely small fraction of its entire orbit. In a telescope or on a photograph a comet appears as a large and hazy object, so that measurements of its position can achieve but a limited accuracy. A comet is very liable in its journey to and from the sun to suffer deflections from its normal path as a result of the disturbing action of the planets; and this further adds to the uncertainty of the calculations. It will be clear, therefore, that the so-called parabolic comets are better regarded as comets whose orbits are indistinguishable from parabolas. And in this connexion it may be mentioned that there are few, if any, comets with periods much in excess of 100 years for which the observations are good enough to make the distinction between a parabola and an ellipse, between a period of indefinite or of definite length. Periods from a few hundred years up to 10,000 years have been occasionally assigned to comets, but only an optimist would attach any significance to them.

We have stressed this point because, if it can be shown that a comet entered the solar system along an orbit which was even slightly on the hyperbolic side of a parabola, we should know that it had come from outside our system and was not an original member of it. Several comets are said to have travelled in hyperbolic orbits; but Professor Strömgren of Copenhagen has shown that for only eight of these were the observations reliable enough to prove definitely that the orbits were not highly elongated ellipses. Moreover, he showed that the eight which did definitely pass round the sun in hyperbolas, were not originally hyperbolic, but rather had their hyperbolism thrust upon them. In each case there had been a close encounter with a planet during the inward journey to the sun; and this had brought about the conversion of an originally elliptical orbit into a hyperbolic orbit. In 1871 Schiaparelli had pointed out that if comets were, so to speak, picked up casually by the solar system in its rapid journey through space, the result would be a large preponderance of hyperbolic orbits. Thus Professor Strömgren's conclusion that there is no evidence for a comet having ever *entered* the solar system along a hyperbola, makes it almost certain that the comets are all original members of the solar system, and travel invariably in ellipses though often with periods of hundreds or thousands of years. A similar conclusion had been arrived at on different grounds by Carrington and Mohn in 1860. If comets are accidently swept up by the solar system, as it moves with a speed of twelve miles a second through space, many more comets ought to meet us than overtake us; and they ought to arrive most frequently from that direction in the heavens towards which we are travelling. It was found however tha comets arrive indiscriminately from all directions.

The vast majority of comets undoubtedly move in ellipses so large as to be indistinguishable from parabolas, a minority travel in definite ellipses with determinable periods from about three years to about 100 years, finally a mere handful are known to have had their orbits converted by the planets into hyperbolas and thus presumably to have left our system for ever. The number of comets that have been seen, including the different returns of the periodic comets, amounts to about 900. Of these about 400 were recorded in the centuries preceding the invention of the telescope. As regards the comets observed in recent times we know from the study of their orbits that the great majority are moving in "practically parabolic" paths with indefinitely long periods. But we can also draw the same conclusion from the records of the pre-telescopic comets. All these were necessarily visible to the naked eye and most of them were very conspicuous objects. If any of the brighter of them had had periods of less than a few hundred years we should have expected to find records of them recurring at regular intervals. And yet there is only one example of a regular sequence, and that is given by the records of the observations of Halley's comet. Moreover, we cannot ascribe the existence of this particular sequence to a lucky chance; for, as shown by Dr. P. H. Cowell and Dr. Crommelin, the records are complete for every one of the twenty-seven returns of Halley's comet (at an average interval of seventy-seven years) right back to 87 B.C., and with one exception to 240 B.C.

Apart from the few comets which *left* the solar system in hyperbolic orbits the remainder, a minority, travel round the sun in ellipses with definitely determinable periods, less than a few hundred years long. In 1925 there were about fifty comets known with periods less than 100 years; and of these seven-eighths had periods between three and nine years, and two-thirds had periods between five and seven and a half years. These short-period comets with periods up to nine years exhibit the remarkable property that the parts of their orbits most distant from the sun come very near to Jupiter's orbit. The chance of this happening by pure coincidence is extremely small; and there can be no doubt that there is some close connexion between Jupiter and these short period comets. Dr. A. C. D. Crommelin of Greenwich, who is probably the leading authority on comets, has long been a strong supporter of the view, first put forward by R. A. Proctor, that such comets actually originated from Jupiter by some volcanic process similar to, though much more violent than, that great eruption at the close of last century when Krakatoa blew great volumes of dust into the upper atmosphere. The majority of astronomers, however, favour a different theory, which was due mainly to the theoretical investigations of H. A. Newton of Yale and of Tisserand and Callandreau in France during the final decade of last century.

They believed that the comets belonging to Jupiter's family, the number of which is now about fifty, were " captured " by Jupiter as a result of his gravitational attraction. If a comet happens to pass close to Jupiter its motion will be either accelerated or retarded according to circumstances. If the comet had previously been travelling in a practically parabolic path, an acceleration would divert it into a hyperbola by which it would leave the system never to return; on the other hand a retardation would produce a more or less considerable shortening of the period of the comet, which would move henceforward in a definite ellipse. Depending on the elongation of the original orbit and on the closeness of the approach to Jupiter, the reduction in the comet's period to about seven years might require one or a number of such encounters.

It must be noticed that there is a definite limit to the extent to which Jupiter can reduce the size of a comet's orbit. For after each encounter (comparatively close approach) the new orbit must of necessity pass through the point at which the encounter occurred. The smallest possible orbit must therefore be one which is still large enough for its furthest point from the sun to reach within a *comparatively small* distance of Jupiter's orbit—otherwise no encounter would have been possible.

But whenever one of Jupiter's family suffers an encounter there is an equal chance that its motion will be accelerated instead of retarded, that the size of its orbit will be increased instead of diminished. In this case, however, there is no limit to the increase in size which may ultimately be produced. Some of the accelerated comets will become diverted into larger ellipses with periods which, though longer than before, will still be of determinable length. Some, after one or more encounters, will be diverted into paths indistinguishable from parabolas with indefinitely long periods. Finally, a few will be diverted into definitely hyperbolic orbits. These last will leave the system and be lost for ever from observation, while those diverted into practically parabolic orbits will no longer be recognized as periodic comets. Only those will continue to be classed as " periodic comets " which still revolve in ellipses that are not too large to be confounded with parabolas. The orbits of these periodic comets of longer period will tend to be rather sparsely distributed over a wide range of periods and sizes; and since many of these comets will have quite long periods and pay comparatively infrequent visits to the sun, the number of them that are known will be further reduced owing to the smaller chance of their being discovered. We thus see that the " capture theory " gives a plausible explanation of Jupiter's family of comets: of the tendency among the periodic comets for the *sizes and periods* of the orbits to conglomerate round a particular size and a particular period (i.e., about seven years); and of the fact that only a small minority of periodic comets are known with periods in excess of about nine years.

It will be seen from the above that the short period comets cannot strictly be regarded as forming a *permanent* Jovian family. The number of them is rather to be looked upon as representing the present level of an equilibrium between those that are being gradually lost from it by deflection into long-period orbits and those which are from time to time added to it by fresh captures.

Two matters must now be mentioned which lend considerable support to the capture theory: one is a further peculiarity exhibited by the orbits of the short-period comets, and the other practically amounts to the actual observation of the capture of a comet by Jupiter.

It is a remarkable fact that all the comets of Jupiter's family revolve round the sun in the normal direction of the " one-way traffic " rule, in the same direction as the planets. On the other hand many of the parabolic comets move in the opposite direction. It is therefore extremely unlikely that this is a mere coincidence. It was shown in 1891 by H. A. Newton that a comet was more likely to be captured by Jupiter if it was moving in the same direction as Jupiter than if it was moving in the opposite direction. One reason for this is that if the two bodies were travelling in the same direction they would remain longer in one another's vicinity than they would if they were passing each other in opposite directions; consequently, Jupiter's gravitational pull on the comet would be acting over a longer time and so produce a bigger effect. If the pull happened to be accelerating the comet, a maximal effect would be more likely to occur; and the comet would be more liable to be removed from the periodic comets *in a single stage* by deflection into a hyperbola. If on the contrary the pull happened to be causing a retardation, the comet would, in the same way, be liable to a more drastic reduction of its period, and so would be more likely to be assimilated into the Jovian family. On the other hand, long period comets moving in the opposite direction to the planets would stand a very small chance either of being permanently thrown out of the solar system or of being captured for Jupiter's family.

In 1889 W. R. Brooks of New York, who must rank among the most prolific comet discoverers in history, found a new comet which, though unimpressive at first sight, soon turned out to be of unusual interest. The comet, which was observed very thoroughly, was found to be a member of Jupiter's family and to have a period of 7·1 years. When, a month after discovery, the comet had come nearer, it was found by Barnard to be attended by four miniature comets. Two of these soon faded away, but one of the others brightened considerably and for a time even outshone the main comet. Brooks' comet has now been observed at four successive returns, but its strange companions have never been seen again. But the most remarkable thing about this comet was its history immediately preceding its discovery in 1889. As soon as the observations were sufficiently numerous for the purpose, Dr. Chandler and Professor Poor proceeded to trace the motion of the comet backwards prior to 1889. They found that on July 20, 1886, the comet had approached so near to Jupiter that it had actually passed inside the orbit of the planet's nearest satellite. They also investigated in detail the manner in which the orbit of the comet had thereby been dislocated, and showed that prior to the encounter the comet had revolved in a much larger ellipse with a period of twenty-nine years. This then was a clear case of a comparatively long-period comet being captured by Jupiter for its family. Moreover it is fairly certain that in the process of being captured Brooks' comet suffered no little violence; and that the four attendants seen during its subsequent visit to the sun had been ripped out of it by the tidal action of Jupiter.

There is no reason why the three other great planets should not capture and retain families of comets; but since the largest of them has less than one-third of the mass of Jupiter, the chances of them doing so are comparatively small. It is usually said that Neptune has six comets, and Saturn and Uranus two apiece; but the connexion between these comets and their respective planets is not nearly so close as that between the short period comets and Jupiter, and is quite possibly a pure coincidence.

There is one comet whose orbital motion is of special interest. This is Encke's comet, whose period of revolution round the sun, 3·3 years, is much shorter than that of any other comet. With the exception of Halley's comet, it was the first comet of which the periodicity was recognized—by Encke in 1823. Since its original discovery in 1786 it has made forty-five returns to the sun and has been carefully observed during most of them. It was Encke himself who first drew attention to certain curious changes in the orbit over and above those that could be ascribed to the disturbing action of the planets; for he found that the comet arrived at its nearest to the sun about two and a half hours earlier at each return. He pointed out that this could be explained by the comet passing through some sort of resisting medium which retarded its progress. This statement doubtless reads like a misprint; but it can easily be shown that if a body moving round the sun is slowed down by friction or some external gravitational pull, it will tend to fall towards the sun and in so doing gather more speed than it has just lost; thereafter it will move slightly faster and in a slightly smaller orbit than before. Encke's suspicions were fully confirmed by subsequent observations. Thus between 1819 and 1914 the period had shortened by no less than two and a half days, while the average distance of the comet from the sun had decreased by over a quarter of a million miles. A thorough investigation of the matter was made by von Asten and Backlund in 1884. They found that the resistance to the comet's motion had in 1868 suddenly diminished by one-half; they also found that the resistance only came into play over a limited stretch of the orbit fairly near the sun. Since then the resistance seems again to have diminished abruptly on several occasions. There can be very little doubt that Encke's suggestion of a resisting medium is correct. Possibly there is a narrow belt of small meteoric particles surrounding the sun, through the midst of which the part of the comet's orbit nearest the sun happens to pass. Any alteration in the form of the orbit would naturally modify the extent to which it was immersed in the meteoric belt; so the time for which the comet was exposed to the resistance would vary from one return to the next. Further, if the meteoric belt consisted of zones of different density, the occasional abruptness in the variation of the resistance would be understandable. Since no other member of the solar system—comet or planet—copies this strange behaviour of Encke's comet, we must suppose the resisting belt to be of very limited extent. It might then be that while this one comet happens to cross it by a fairly long diagonal track, other comets cross it directly or miss it altogether.

As to the origin of comets we know nothing. It may be that they were ejected from the planets long ago by some volcanic process, or they may have been formed directly from the sun at the same time as the planets, with the material that was, so to speak, left over.

There are probably more mysteries that are completely unexplained in connexion with the comets than with any other kind of celestial body. One reason for this, as we have said, has been the scarcity of bright comets in recent years: there has been no opportunity for really detailed study by the most modern methods. But *when* the opportunity will come, and *when* the next great comet will appear, we cannot tell. For all the great comets with one exception have periods so long that they are quite unknown. The exception is Halley's comet and that is not due back until about 1987.

CHAPTER XXIX

SHOOTING STARS

IN 1899 an unfortunate thing happened. An astronomical prediction to which the press had given the widest publicity completely failed to come off. The public, led to expect much, saw nothing; and astronomy, which they believed could foretell, with absolute certainty, events centuries ahead within a fraction of a second, lost much of its high reputation. The expected event was the return of the great shooting star shower of 1833 and 1866. Records of many of its returns at this same interval had been traced back to A.D. 902 and there had seemed no reason to suppose that it should now suddenly fail. It is true that Johnstone Stoney and A. M. W. Downing, Superintendent of the Nautical Almanac Office, had realized the possibility of disappointment and issued a warning to that effect; but their last minute efforts quite failed to check the growing excitement or to mitigate the resentment that was afterwards felt. It appeared from the investigations of these authors that the portion of the swarm we had traversed in 1866 had in the meantime suffered considerable deflection at the hands of Saturn and Jupiter. But whether on this account the shower would fail to materialize, would clearly depend on the volume of space which the main swarm occupied—a point upon which there was complete ignorance.

Since the disaster of 1899 astronomers have approached the prediction of shooting star showers with greater humility. The return of the same swarm in 1933 was examined in very great detail by Dr. Crommelin. He found that since 1899 its orbit had again been deflected by the planets, so that it once more lay close to the earth's orbit. But as there was a considerable interval between the arrival of the swarm and the arrival of the earth at the crossing point, it was impossible to say whether a super-display of shooting stars would occur: everything would depend on the unknown extension of the swarm. Actually the November shower of 1933 and of the years preceding and following it were only slightly above the average. There is, however, no reason to suppose that the swarm, which for upwards of a thousand years has periodically terrified or delighted humanity, has worn itself out. It is simply a question of " hit or miss "; and though we *missed* the last two times, we may have better luck in 1966 or 1999.

The shooting stars so far considered have belonged to well marked annual showers and have been shown to be connected with known comets. But apart from the few showers known to be connected with comets, there are a great many others more or less well defined for which no such connexion has been demonstrated. The points characteristic of a shower are these: (1) the meteors belonging to it all appear to radiate—like the spokes of an umbrella—from one small region in the heavens; (2) the shower takes place on the same date or dates—for it may continue over *several* days—each year, and each year the point of radiation in the sky (or " radiant ") is the same; (3) the colour and speed of the meteors are characteristic of the shower and, since the faster a meteorite travels through the air the hotter it becomes, it follows that the sluggish meteors will be red and the fastest meteors will be bluish white.

The man who probably contributed more than any other to our knowledge of meteors during the end of the last and the beginning of the present century was W. F. Denning of Bristol, one of the greatest of amateur astronomers. He it was who finally proved that the point from which the August shower radiated was not absolutely fixed in the sky but moved very slightly from night to night. That it should do so was a necessary consequence of the earth's orbital motion as it traversed the meteor swarm—provided of course that the accepted theory of meteor swarms was correct. It was thus of capital importance that the motion should be demonstrated by observation; and this the precise and exacting observations of Denning succeeded in doing. As early as 1876 he published a catalogue of twenty-seven annual showers with the positions of their " radiants," and by 1923 he had increased this number to no less than 314, all derived from the Bristol observations. Finally from the combined observations of himself and others he gave a catalogue of 4,000 separate radiants, many of which however he regarded as to some extent uncertain. It is clear therefore that the number of meteoric swarms that cross the earth's orbit in their revolutions round the sun is very great; presumably the number of those that do not cut the earth's orbit, and of which we must ever remain ignorant, is enormously greater. There is not a single night in the year without one or more showers being in progress; and though many of them are weak and only to be detected by careful watching, others not uncommonly give an hourly yield of fifty or more shooting stars. Dr. C. P. Olivier, Director of the Flower Observatory, Philadelphia, and President of the Commission on Meteors of the International Astronomical Union, has lately listed 1,200 separate showers and regards the reality of at least half of these with confidence.

In spite of the large number of different showers that have been definitely detected, the large majority of shooting stars are quite independent bodies and show no communal tendency to radiate from a particular point in the sky. These are called " sporadic meteors."

The reader will probably be surprised that shooting stars are so numerous: that several showers are in progress each night of the year, and that on top of these there is a still larger number of haphazard shooting stars. How comes it that he has not noticed them? The reason is that most shooting stars are faint and only to be caught in their rapid flight if one is keeping a strict look-out. There must be no moon and the sky must be clear and free from the reflection of city lights. Finally, since meteors are much more likely to meet than overtake the earth, as it rushes at eighteen miles a second round the sun, shooting stars are much more numerous after midnight, when the part of the heavens towards which the earth is travelling comes up above the horizon.

The observation of meteors is done almost entirely with the naked eye. They are gone so quickly that it is impossible to observe them with a telescope, except on the rare occasions when one just happens to cross the field of view. On occasions, and for special purposes, photography has been used; but even the fastest lenses can only record the brighter meteors; and nights or weeks may pass before a bright enough meteor will turn up in the direction towards which the camera is directed. The objects of the naked-eye observer are these: (1) to note and record the exact path of the meteor among the stars and to chart the path on a map; (2) to note the approximate time of the appearance of the meteor and its brightness; and (3) to determine the number of seconds and fractions of a second occupied by the meteor in its flight. If a shower is in progress the first piece of information will enable even a single observer to determine the position in the sky of the radiant. The larger the number of meteors he manages to observe the more accurately will this position be found; and if he can combine with his own observations those of others, the accuracy will be still greater. Further if *two* observers some miles apart manage to record the track in the sky of the *same* meteor, it is possible to work out the actual path which the meteor followed *in relation to the ground* together with the *length* of the path and the *heights* above the ground of its beginning and ending. It is for this purpose that the times and brightnesses of the meteors are noted by the observers, for otherwise the identification of a given meteor would be well-nigh impossible. Finally, the third observation, the estimated *duration* of its flight, gives, when combined with the calculated length of its path, its actual velocity in miles per second. This estimate of duration is the most difficult and uncertain of all the observations, with the result that our knowledge of the velocities of meteors—so important as we shall see in a moment—is still depressingly inadequate.

The study of meteors—and specially the study of their real paths, heights and velocities, when each meteor must be observed by two or more distant observers—obviously requires a great deal of careful organization. The realization of this important point has resulted in the formation during recent years of societies, mainly of amateur astronomers, for the express purpose of meteor observation. The best known of these are the Meteor Section of the British Astronomical Association and the American Meteor Society; and there are others in Russia, in Germany and at Prague.

It is found that the average shooting star first becomes visible at a height of about seventy miles, and after traversing a path thirty to forty miles long is finally burnt out at a height of about fifty miles above the ground. The faster moving meteors first appear somewhat higher, about eighty to one hundred miles above the ground; also they are usually burnt out at a higher level than the slower moving bodies. There are two reasons for this difference: the density of the atmosphere required to produce sufficient friction for the conflagration will be less in a fast-moving meteor; also the faster a meteor is moving the more quickly will it be completely burnt out. Very bright meteors, or fire-balls as they are called, generally traverse a much longer track than an ordinary shooting star. The reason is that a fire-ball is a much larger body and thus has time to get much further before it is burnt up; in fact we have already seen that the larger meteoric bodies of this kind quite frequently land on the ground.

The velocities of meteors, got by the simple method given above, range from about fifteen to seventy-five miles a second. These are, however, notoriously inaccurate. In the last few years attempts have been made to obtain more or less precise measurements of velocities by means of photography. An ingenious device, used on rare occasions at various times and by various observers during the last forty years, is a rapid camera with an occulting-shutter revolving in front of the lens. The shutter whose revolutions can be accurately timed, is arranged to interrupt the exposure two or three times each second. If then a meteor is caught on the plate, the long trail which it makes will be seen to be broken at regular intervals. An arrangement must previously have been made with a second observer at a distance to watch, or better photograph, the sky at the same time. The length in miles of the meteor path is calculated in the ordinary way from the two observations. The time occupied by the meteor in its flight is given by the number of interruptions in the trail on the photograph. Finally, the length divided by the time gives the meteor's velocity. The great disadvantage of this, as of all photographic methods, is that many hours will have to be spent in getting even one successful result. That is the reason why the method has been so little used.

The observed velocity of a meteor is clearly its velocity relative to the earth. If therefore we require the " real " velocity of the meteor—its velocity relative to the sun—we shall have to allow for the velocity of the earth in its orbit which is eighteen-and-a-half miles a second. If a meteor is overtaking the earth directly from behind we shall have to add eighteen-and-a-half miles a second to its observed velocity in order to get its real velocity. Conversely if a meteor meets the earth in a head-on collision we must subtract the eighteen-and-a-half miles a second from the observed velocity to get the real velocity.

Now the determination of the real velocity of a meteor is important because it tells us whether the body in question is a permanent member of the solar system, or whether it comes from outer space and is just by chance passing through the solar system. For a body which is revolving permanently round the sun in an ellipse cannot, when it gets as far out from the sun as the earth's orbit, travel faster than twenty-six miles a second. On the other hand if a body in the neighbourhood of the earth's orbit *is* moving faster than twenty-six miles a second, it must be travelling in a hyperbolic orbit and so must be in process of paying a purely incidental visit to the solar system—a casual visitor from outer space.

As would be expected all meteors belonging to annual shooting star showers have real velocities that never exceed twenty-six miles a second, although their velocities as observed, relative to the earth, may reach nearly forty-five miles a second if they run into us head-on. This fact confirms the theory already well established that such meteors belong to swarms of meteorites that are permanent members of our system revolving round the sun in ellipses.

But with the sporadic meteors it has long been suspected that the rule of the 26-mile limit is not rigidly observed. Undoubtedly many of the sporadic meteors do keep within the limit and are isolated particles revolving round the sun in ellipses. Some of these may be the remnants of completely disintegrated swarms; others may be particles left over at the birth of the planets which just happen never to have been collected up into definite swarms. Further, if any did really exceed the limit it was possible that they had been diverted into hyperbolic orbits by recent close encounters with one of the planets. But this explanation could only account for an *occasional* hyperbolic meteor; and it was therefore highly desirable to find some method for determining the velocities of *large numbers* of sporadic meteors in order to get some idea of the frequency with which they exceeded the speed limit.

This problem was finally taken in hand and solved largely as the result of the enthusiasm and industry of Professor Shapley of Harvard. From October 1931 to July 1933 an elaborately planned expedition from Harvard College Observatory was stationed near Flagstaff, Arizona. The site was chosen because of its excellent weather conditions which insured the possibility of long spells of continuous observation; and because of its high altitude, nearly 7,000 feet, which gave very transparent skies and permitted the observation of fainter and more numerous meteors than would otherwise have been practicable.

The real paths and heights of meteors were studied by means of simultaneous observations from two stations, one actually in the grounds of the Lowell Observatory and the other about twenty-two miles distant. In order to increase the number of meteors observed there were usually two observers at each station who divided the sky between them. Instead of noting the track of the meteors *directly* in relation to the stars, the observers made use of "reticules," specially designed for the expedition. The "reticule" was a sort of shelter, across the open roof of which a grid of coarse wires was stretched. The observer sat within the reticule and kept his eye at a fixed point near the centre of it, and noted the commencement and ending of each meteor track in relation to the wires of the grid. It was found that by this method the observations of the real paths and heights of the meteors were greatly increased in accuracy.

Another quite new and most ingenious device was that for determining the velocities of the meteors. It was invented by Dr. Öpik who was in charge of the expedition. This consisted of an ordinary flat mirror mounted in such a way that it could be given by a motor a rapid circular rocking movement. By placing the eye quite close to its edge and looking at the sky reflected in it, the observer saw the stars drawn out by the motion into small ellipses. But when a meteor dashed across the mirror it appeared as a wavy line, the shape of which was very sensitive to the apparent velocity with which the meteor was moving. It was found, moreover, that if the observer immediately made a free-hand drawing of the form of this wavy line, he could afterwards determine from it very accurately the apparent velocity of the meteor. If this same meteor had been observed by the reticule-observers at the two stations, so that the real length of its path could be calculated, its actual velocity in miles per second followed immediately.

The result of this unique expedition was to show that no less than ninety per cent. of the sporadic meteors had real velocities definitely in excess of the 26-mile limit. In other words, the great majority of the sporadic meteors are travelling in definitely hyperbolic orbits and have nothing whatever to do with the solar system. They represent particles of the rubbish scattered about interstellar space, which are continually passing through the solar system as they and it pursue their appointed journeys through space.

We have seen that occasionally a large meteoric body may survive its journey through the air and reach the ground. The chances of its survival are mainly dependent on two factors. The larger it is the greater is the chance of its reaching the ground before it is completely vaporized. Again, the slower it is moving through the air the smaller is the chance of its being burnt up before it reaches the ground. Every year two or three meteorites are found after being seen to fall; many more, however, which are seen to fall are never found; and finally a vastly greater number must fall to the ground without even their flight through the air being recorded. When picked up a meteorite has usually a characteristic appearance. It has a smooth, glossy surface owing to the fusion by heat of its surface-layers. Also it shows curious indentations, like those made by one's thumb in a plastic substance, where portions of more readily fusible material have been melted away. Meteors consist mainly of stone or iron—sometimes in equal amounts, sometimes one or other greatly preponderating—together with small quantities of other substances. When heated, meteorites give out, among other gases, carbon-monoxide and hydrocarbons, the gases that are typically found in comets. Apart from the meteorites which are seen to fall, and are found as a result of a deliberate search, there are others which may have fallen centuries ago and which are recognized by their typical appearance.

The meteorites that have been collected have weights ranging from a few ounces to several tons. One of the largest, which was found by Admiral Peary at Melville Bay, Greenland, and is now in New York, weighs 36½ tons and measures about eleven feet, by seven feet, by five feet. Quite often several small meteorites are found close together on the ground, and sometimes the number is large, amounting on one occasion to more than 100,000 fragments. Since meteors are frequently seen to explode or undergo a succession of explosions during their flight, it is probable that many of the multiple meteorites found originate from large single meteorites that exploded just before reaching the ground. On the other hand the appearance of the individual fragments sometimes indicates that they were already separate bodies before they entered the earth's atmosphere, that they were members of a miniature swarm travelling together through space. That this may occasionally happen is proved by a curious phenomenon observed in 1913, when between ten and twenty groups of meteors, each consisting of thirty or forty members, were seen traversing practically the same path for 6,000 miles from Canada across Bermuda.

We know of two occasions on which the fall of a great meteorite produced upon the earth's surface a very considerable effect. For one of these the evidence is absolutely definite and for the other it is to say the least extremely strong. The first occasion was several thousand years ago and was staged in Arizona; the second occasion was in 1908 and was staged in Siberia. In north-east Arizona there is in the desert a curious circular crater about three-quarters of a mile in diameter surrounded by a precipitous wall. The floor of the crater is about 450 feet below the level of the surrounding desert; and the wall rises to a height of 600 feet above the floor and 150 feet above the surrounding plain. In form therefore this crater in no way resembles a volcanic crater; and there is no evidence of volcanic activity for many miles around. The walls consist of great lumps of limestone and sandstone piled on top of one another and obviously cast up from the floor of the crater. Artificial borings into the floor of the crater show that the rocks beneath it have been crushed to a depth of several hundred feet. Moreover, these rocks show definite signs of having been heated at some time to a very high temperature. But there were two findings which were even more significant: specimens from the borings contained particles of the mixed oxides of iron and nickel, a combination practically only found in meteorites; and within a radius of five miles of the crater thousands of iron meteorites were discovered scattered over the desert.

There can be practically no doubt that " Meteor Crater " as it is called was produced by the impact of a very large meteorite, or of a very large and dense mass of meteoric material. The thousands of meteorites lying around the crater may have been outriders of the main mass, or they may have been scattered there by an explosion of the main meteorite as it hit the ground. It is unfortunate that the exploration of the crater has been unavoidably held up since 1927; but it is hoped that it will be continued and carried to completion in the near future. It is not yet certain whether a large meteoric mass will be found buried beneath the floor, or whether, as is more probable, the original meteorite—a single body or a compact swarm—was scattered widely in fragments by the explosion of its impact.

On the night of June 30, 1908, and on the next few nights there was visible in England after sunset a strange pink luminosity of the sky which persisted through the short hours of darkness into the dawn. Attention was drawn to it in the press and there were many people who witnessed it during the remaining nights of its visibility. It was suggested at the time that the cause of the phenomenon was a violent volcanic eruption, for somewhat similar appearances had been noticed all over the world after the eruption of Krakatoa in 1883. But when the days, the weeks and finally the years went by without any news of an eruption or an earthquake, people began to forget about the luminous phenomena of 1908. It was not until twenty years later that the matter again came up for general discussion. In the meantime, soon after the war, vague rumours began to percolate into Russia that something unusual had happened several years earlier in central Siberia. An intensely brilliant object had been seen rushing across the sky in full daylight, violent explosions had been heard and sudden winds and hot blasts of air had been felt—such were the bits of information which travellers from Krasnoyarsk had picked up from natives living in the country round. In 1921 Dr. L. A. Kulik of the Academy of Sciences at Leningrad set out to investigate the rumours at first hand. He went to the neighbourhood of Krasnoyarsk and spoke with many people who had witnessed something of the phenomenon themselves or who had heard about it from eye-witnesses. The sudden blast of wind had in some cases been sufficiently strong to fell horses to the ground and throw people off their feet although the site of the explosion was believed to be over one hundred miles further north. Curious waves flowing upstream in the rivers had been noticed; and those who had seen the flaming body said it was bluish-white in colour. But there was other information related to Dr. Kulik at second hand; for natives coming from the sparsely inhabited regions some 500 miles north-east of Krasnoyarsk told of the widespread destruction of reindeer and the uprooting of large tracts of forest. He was given to understand that it would be possible to find natives who would be able to conduct him to the devastated region, but since the only road was now blocked by the upturned trees the journey would be difficult.

In 1927 a second expedition was sent out under the leadership of Dr. Kulik. After a journey of three months, made mostly by rafts along small rivers, he finally reached the area of devastation which lay in the midst of a marshy forest. For a distance of nearly forty miles surrounding the centre of this area trees were uprooted and lying on the ground; and within a radius of twenty miles of the centre the uprooting had spared scarcely a single tree. The trees within the devastated area were scorched as by a great heat. But the strangest matter of all was that all the trees lay with their tops directed away from the centre of the area. Looking at them from near at hand one saw endless rows of trees with their trunks all parallel to one another; looking down on them from one of the small hills in the vicinity one saw that they radiated outwards from a centre like the ribs of an umbrella. Moreover, in the swamp among the fallen trees were numbers of holes full of water up to fifty yards in diameter.

Dr. Kulik visited the place again in 1928 and 1929, and on this last occasion spent nearly two years investigating the phenomenon. The results show that the devastation took place about 6 a.m. (a quarter of an hour after Greenwich midnight) on June 30, 1908. It was a warm sunny morning without a cloud in the sky; and those who heard only the thunder commented upon it on that account. The body must have been intensely bright; for two witnesses who were at least thirty miles from the meteor when it passed them felt a momentary scorching heat from it. Other witnesses at greater distances compared the brightness of the body with that of the sun. One native had previously used the devastated area as a pasture for his reindeer and had built there many sheds in which he kept various implements and clothing. Shortly after the great explosion, when he visited his camp, everything was burned up and melted to pieces; and of only a few of the reindeer were the charred carcases discovered. Thus there can be no doubt that the blazing body, or meteor, was the direct cause of the devastation.

In spite of most careful searching Dr. Kulik was never able to find any definite meteorites or meteoric fragments either in the " shell holes " or on the ground around. It is possible that the meteor consisted of a dense swarm of small particles which were mostly vaporized before reaching the ground. The meteor must have been travelling at a very high velocity through the air, as was shown by its blue-white colour, on which all were agreed. It was also probably very large: judging from the descriptions of those who saw it pass in the sky at a great distance and compared it in size with the sun and moon, its diameter must have been of the order of a mile and possibly a good deal larger. Its effect would have been that of a gigantic piston plunging through the atmosphere and compressing tremendously the air in front of it. As it hit the ground the compressed and superheated mass of air and meteoric vapours would have escaped sideways in all directions flattening out and scorching the forest for miles around. It might well be that any meteoric particles that were not vaporized were blown by the immediate re-expansion of the air to great heights in the atmosphere. That might explain the failure to find any meteoric material in the neighbourhood, and also the luminous phenomena seen all over Europe on the following nights, so similar to the appearance caused by the high-blown dust from Krakatoa.[1]

It was thought at one time that this meteor was a part of Pons-Winnecke's comet, whose orbit we were very close to at the time. But Dr. Crommelin has pointed out that the meteor, which all agree travelled from south to north, was moving in exactly the opposite direction to that required by such a theory. The apparently very large velocity of the meteor suggests that it was moving in a hyperbolic orbit and so was not a member of the solar system but came from outer space.

It is fortunate that this meteorite landed where it did. In a populated region it would have done appalling damage. The force of the impact and explosion was terrific: eighty million trees were uprooted; the noise of the explosion was heard over 600 miles away; the earth tremor due to the impact was recorded as far off as Jena in Germany; and, as Dr. Whipple has shown, the pulse produced by the sudden compression and re-expansion of the air recorded itself on numerous barographs in England. If this same meteorite had fallen on London, instead of in a Siberian forest, it is probable that no building would have been left standing and no person left alive, except in the more distant suburbs. Fortunately such events are excessively rare, quite unexpected and all over in an instant. Apart from the Siberian meteorite of 1908 and the Arizona meteorite of several thousand years ago, we know definitely of only four other events that are at all comparable. On four unknown occasions in the past meteoric craters (similar to, though much smaller than, the Arizona crater) have been produced: in Texas, Central Australia, Arabia and the Argentine.

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[1] But this would imply a velocity of 300 miles an hour for the dust in the upper atmosphere; the corresponding velocity at the Krakatoa eruption was 73 miles an hour.

CHAPTER XXX

CO-OPERATION AMONG ASTRONOMERS

THE rapid strides made by astronomy during the last hundred years are largely the result of new instrumental and theoretical methods of investigation. But another factor responsible for this progress, the importance of which cannot be overestimated, is the increasing tendency for co-operation among astronomers, co-operation both individual and international.

Probably the first attempt at international co-operation in astronomy was the scheme set on foot by Bessel in 1824 for the visual charting of the whole sky. It was expected to require four years, but actually it was completed in 1858. The thirty years' delay was urged by many as a strong argument against further efforts of the kind; but fortunately there were some who thought differently.

The first of the big international conferences was the meeting in Paris of the Conférence Diplomatique du Mètre, originally intended for 1870, but postponed till 1875 as a result of the Franco-Prussian War. Out of it arose a permanent international bureau, which in subsequent years did much valuable work by advising on the definition of precise standards of measurement, and on the accurate determination of various physical and geophysical constants. But the geophysical aspect grew rapidly in importance and soon required treatment on its own. And thus came into existence the International Geodetic Union whose first meeting was held in Rome in 1883, and whose 1884 meeting in Washington is memorable for the final adoption of the Greenwich meridian as the zero meridian for all countries.

Perhaps the most important of the pre-war international organizations was that initiated in Paris in 1887, at the instigation of Admiral Mouchez, for planning and carrying into effect the Carte du Ciel. Unfortunately this immense project—the photographic charting and cataloguing of the entire sky to include the fainter stars—was seriously held up by the Great War, and is still not quite completed.

Two other organizations of a similar kind were the International Solar Union and the Commission Internationale de l'Heure. The Solar Union started with a meeting at St. Louis in 1904 and met in subsequent years at Oxford, Meudon, Mount Wilson, and

Bonn. In 1916 it was to have met in Rome, but this was prevented by the war. The Commission Internationale de l'Heure was founded at a meeting in Paris in 1912 which had been called to deal with the matter of wireless time-signals. It drew up many important recommendations as to the standardization of time-signals and as to the accuracy which they should achieve; and it was to have considered and advised upon the various methods of radiating and receiving signals. But the work of the commission came to a premature end, for during the war the general employment of wireless telegraphy was prohibited in most countries.

Now each of the pre-war organizations for international co-operation was concerned with its own particular branch of astronomy; for they had come into being one at a time, as the necessity for them had been felt. It was clear, however, that it would be much more convenient for every one concerned if they could be brought together into a single body. And so, when these various organizations died a natural death as the result of the war, it was decided to revive them, not in their original form, but as branches of an all-embracing International Astronomical Union. This was accomplished at a meeting in Brussels in 1919 of the leading scientists of the allied countries; and in 1922 the first meeting of the International Astronomical Union was held in Rome. Since then meetings of the " I.A.U." have been held at intervals of approximately three years, in Cambridge, Leiden, Harvard, and Paris—the next meeting, in 1938, being due to take place in Stockholm.

At first the I.A.U. consisted of thirty-two separate " commissions," fourteen of which could be regarded as the direct descendants of pre-war organizations. By now the number has increased to thirty-six. At present there are twenty-four different countries adhering to the Union; but astronomers from countries not adhering can attend meetings as visitors and take part in the discussions without voting. The work of the Union consists mainly in organization: in deciding what problems are most urgently in need of investigation; in considering the best methods for such investigations; and in dividing up the work involved among observatories. In astronomy there is a tremendous amount of more or less routine work, as regards both observation and computation; and in the past, through lack of proper co-operation, such work has often been unnecessarily duplicated. By apportioning such work among different observatories and institutions and by arranging for the free interchange of data and results, the Union has done much towards economizing time, labour and money.

The Union has on occasions helped to finance the publication of important material; and it has itself produced many valuable reports on work that is planned, completed or in progress. But apart from these official functions of the Union, there are others which, though incidental, are equally valuable. For by its meetings it brings together astronomers from all over the world, who get to know one another and discuss their common problems and mutual interests.

Another international, or partially international, organization is the Astronomische Gesellschaft, which was founded in Heidelberg in 1863. It is probably the only international organization that survived the war entirely in its original form. The international activities of the Astronomische Gesellschaft have been mainly concerned with variable stars and stellar photometry.

The great majority of the astronomical societies of the various countries are occupied with the reading, discussion and publication of original astronomical papers; they do not concern themselves with the organization and collation of observations. Only indirectly through their meetings, where astronomers come together and exchange ideas, do they facilitate mutual co-operation among observers and mathematicians. But there are a few societies of which the main object is to carry out a definite programme of work, the members of the society collaborating under a director. The oldest of such bodies is the British Astronomical Association founded in 1890. Besides carrying on the ordinary activities of a scientific society—the holding of meetings and the reading of original papers—it supports a number of " Observing Sections " controlled by " Directors." There are sections for the observation of variable stars, of the various planets, of comets, of meteors, of the sun and of the moon; and there is a computing section. Memoirs are periodically brought out by the various Sections containing the more important observational results. The only other societies of this type confine themselves to a single branch of astronomy. The most important of these are the American Association of Variable Star Observers, the corresponding French society, and the American Meteor Society. It is interesting to note that the majority of the members of these four observing institutions are amateur astronomers.

A hundred years ago our knowledge for all practical purposes did not extend beyond the solar system, beyond a distance from the earth of less than three *light-hours*. And yet to-day we have acquired a considerable store of information out to a distance of many millions of *light-years*. Though much of this new knowledge has been finally accepted and can be looked upon as a permanent addition to our science, a great deal of it is still more or less uncertain or speculative. It will indeed be interesting to see how many of the things which astronomers have learned during this last century will have to be unlearned in the next.

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A Popular Guide to the Heavens, Ball, revised Phillips. George Philip, 1925.

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Half Hours with the Telescope, R. A. Proctor, revised Steavenson. Longmans Green, 1925.

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The Solar System and its Origin, Russell. Macmillan, 1935.

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The Dawn of Astronomy, Lockyer. Cassell, 1894.

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Biography of Percival Lowell, A. Lawrence Lowell. Macmillan, 1935.

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CHRONOLOGICAL TABLE

1834—1937

1834. J. Herschel's landing at the Cape.
1837. Map of the moon by Beer and Mädler.
1838. First determination of a star's distance (61 Cygni) by Bessel.
1839. Invention of first two photographic processes by Daguerre and Fox Talbot.
1842. Doppler predicts principle of colour-change due to radial motion.
1842. First total solar eclipse to be scientifically observed.
1844. Existence of a companion of Sirius predicted by Bessel.
1845. Completion of Rosse reflecting telescope. Its discovery of spiral nebulae.
1845. First daguerreotype of sun by Foucault and Fizeau.
1846. Discovery of Neptune ; position predicted independently by Leverrier and Adams.
1847. Struve finds evidence for considerable absorption of light in space.
1848. Roche's limit.
1849–50. First laboratory determinations of velocity of light, Fizeau and Foucault.
1850. Daguerreotype of moon by Bond.
1851. Schwabe's discovery of sun-spot period published.
1852. Coincidence of magnetic and sun-spot periods announced by Sabine.
1853. Helmoltz' " contraction theory " of the source of solar energy.
1853. First use of wet-plate process in astronomy, for photography of moon, by de la Rue.
1854. Pogson's ratio suggested for standardization of stellar magnitude scales.
1856–7. Invention, independently by Steinheil and Foucault, of process for silvering glass surfaces for telescope mirrors.
1857. Clerk Maxwell shows theoretically that Saturn's rings must be composed of myriads of small particles.
1858. Routine solar photography started at Kew.
1859. Principles of spectrum analysis established by Kirchoff and Bunsen.
1859. Carrington's discovery of the equatorial acceleration of sun's rotation.
1859. Chemical composition of sun's atmosphere described by Kirchoff.
1860. Total solar eclipse : photography shows prominences to belong to sun and not to moon.
1861. Kirchoff's map of solar spectrum.
1861. Babinets' objection to the "nebular hypothesis" on grounds of insufficient angular momentum.
1862. Alvan Clark's discovery of the companion of Sirius.
1862. Sun's distance determined from the velocity of light by Foucault.

1862. Completion of Argelander's *Bonner Durchmusterung.*

1862. Huggins' discovery of a dark line of unknown origin in Jupiter's spectrum.

1862. Kelvin shows that Helmholtz' "contraction theory" would give only 50 million years for life history of sun.

1863. Secchi's classification of stellar spectra.

1864. First spectroscopic observation of a comet by Donati, showing bright emission lines.

1864. First spectroscopic observation of a nebula by Huggins, showing bright emission lines thus indicating origin from glowing gases.

1866. Identity of the orbits of August meteors and of a comet observed in 1862, demonstrated by Schiaparelli.

1866. Appearance of Nova Corona Borealis, first nova studied spectroscopically.

1867. Discovery in Cygnus of stars with bright spectral lines, Wolf and Rayet.

1868. Total solar eclipse: observation of bright line prominence spectrum.

1868. Janssen and Lockyer independently discover spectroscopic method for viewing prominences in ordinary daylight.

1868. Discovery of prominent line of unknown origin in chromospheric spectrum. Lockyer suggests it is due to a new element which he names "helium."

1868. Huggins first employs the Doppler principle to measure radial velocities of stars.

1868. Huggins identifies bright lines in spectrum of a comet with those due to hydrocarbon gases.

1869. Total solar eclipse: detection of bright lines in corona spectrum.

1869. Secchi discovers several dark bands of unknown origin in spectrum of Uranus.

1869. Lane's pioneering investigations as to the internal constitution of stars.

1870. Proctor points out that a number of the brighter stars in the sky are moving together through space.

1870. Total solar eclipse: Young discovers flash spectrum, a bright line replica of the ordinary dark line solar spectrum.

1871. Total solar eclipse: Janssen observes dark line solar spectrum, apparently reflected from the corona.

1871. The earliest determination of galactic rotation by Gyldén, from proper motions of the stars.

1872. Solar spectrum photographed by Draper.

1874. Routine solar photography started at Greenwich, being transferred from Kew.

1874. Vogel's classification of stellar spectra.

1874. Transit of Venus observed by many expeditions for determining the sun's distance.

1876. Nova Cygni. First nova in which nebular-metamorphosis was observed.

1876. Huggins introduces dry-plate process into celestial photography, and gets the first promising photographs of stellar spectra.

1877. Close approach of Mars:—Gill's expedition to Ascension Island to determine the sun's distance from observations of Mars. Schiaparelli's observations of Mars' surface and his discovery of the "canals."

1877–9. Pickering's early results in precise stellar photometry.

1878. Lockyer propounds theory of "celestial dissociation," thus foreshadowing the modern theory of ionization.

1878. Publication of Schmidt's map of the moon.

1879. Pickering designs the Harvard Meridian Photometer.

1879. Darwin's early investigations on tidal friction in relation to the history of the earth-moon system.

1879. Huggins photographs the ultra-violet spectra of white stars, and discovers therein the remarkable rhythmical series of hydrogen lines.

1879. Lockyer starts the observation of sun-spot spectra at South Kensington.

1880. Pickering determines the relative dimensions of the system of the eclipsing double-star Algol, from a study of its light variations.

1881. First photographs taken of spectrum of a comet by Huggins and Draper.

1882. First photographs of spectrum of a nebula (Orion) taken by Huggins.

1882. Bright metallic lines observed in both Comet Wells and the Great Comet when nearest the sun.

1882. Great Comet transits the sun's disc, but no evidence of its silhouette could be detected.

1882. Gill's successful photographs of Great Comet with dense background of stars, taken with small hand-camera, suggests use of photography for charting the heavens.

1882. Huggins unsuccessfully attempts to photograph corona without an eclipse.

1883. First really successful direct photographs of a nebula (Orion) by Common.

1883. Catalogue of the spectra of 4,051 stars by Vogel.

1884. The *Harvard Photometry* published, giving magnitudes of 4,260 stars.

1884. Backlund's researches into the irregular motion of Encke's comet.

1885. Publication of *Uranometria Nova Oxoniensis*: magnitudes of 2,784 stars.

1885. Commencement of photographic work for *Cape Photographic Durchmusterung.*

1885. Super-nova appears in Andomeda nebula; spectrum observed visually.

1886. First photographs of stellar spectra taken with objective prism camera at Harvard.

1886. Photographic work started at Harvard for the *Draper Catalogue* of stellar spectra.

1886. Photographic measurements of stellar distances undertaken by Pritchard.

1886. Langley investigates extreme infra-red solar spectrum with the bolometer which he invented in 1880.

1886. Photographic detection of bright hydrogen lines in spectra of long-period variables.

1887. Michelson's first experimental demonstration of the absence of an ether-drift.

1887. Lockyer's "meteoritic hypothesis"—a two-way theory of stellar evolution.
1887. Completion of Lick 86-inch refractor.
1888. Publication of Kustner's discovery of the variation in latitude.
1888. Vogel's first spectro-photographic determinations of stellar radial velocities.
1888. Heliometric observations of Iris for determining the sun's distance, organized by Gill.
1889. Discovery of spectroscopic double stars by Miss Maury.
1889. Eclipse theory of Algol confirmed spectroscopically by Vogel.
1889. Completion of photographic work for *Cape Photographic Durchmusterung.*
1889. Draper Memorial photographs of stellar spectra extended to southern hemisphere.
1889. Gill's heliometric measures of Victoria and Sappho for determining sun's distance.
1889. Foundation of the Astronomical Society of the Pacific.
1889. First photographs of Milky Way taken by Barnard.
1889. Spectroheliograph invented by Hale.
1889–91. Bailey's work in Peru on southern stars for *Harvard Photometry.*
1890. Foundation of British Astronomical Association.
1890. Publication of *Draper Catalogue* giving spectra of 10,851 stars.
1890. Michelson designs first stellar interferometer.
1891. Capture theory of comets developed by Callandreau, Tisserand and Newton.
1891. Harvard establishes permanent southern station at Arequipa in Peru.
1891. Meeting in Paris to discuss the photographic charting of the heavens.
1891. Wolf makes first photographic discovery of a minor planet.
1892. Work started on the *Astrographic Chart.*
1892. Anderson discovers Nova Aurigae: first photographs of the spectrum of a nova.
1892. Barnard makes first photographic discovery of a comet.
1892. Monck finds proper motions larger for late than for early type stars.
1892. Ranyard derives an excessively low maximum density for Orion nebula.
1893. Kapteyn's statistical studies of the structure of the stellar system.
1893. Mrs. Fleming makes the first of her spectro-photographic discoveries of novae.
1894. Publication of Vol. I of *Potsdammer Photometrische Durchmusterung.*
1894. Establishment of Lowell Observatory at Flagstaff, Arizona.
1894. Campbell points out that the various spectral images of a planetary nebula differ in diameter.
1895. Publication of Bailey's extension of the *Harvard Photometry* to the south pole.
1895. Bailey's discovery photographically of variable stars in star clusters.
1895. Ramsay's discovery of helium in the laboratory.
1895. Keeler's spectrographic confirmation of Clerk Maxwell's theory of the constitution of Saturn's rings.
1895. Schwarzschild's invention of the "coarse grating," and his use of it in the measurement of double stars.
1895. Appearance of super-nova in Centaurus, spectrum photographed.

1896. Publication of part 1 of both the Paris and Lick photographic atlases of the moon.

1896. Effects of pressure on the positions of spectral lines described by Humphreys and Mohler.

1896. New series of " hydrogen lines " discovered by Pickering, in stellar spectra.

1896. Zeeman's discovery of the effect of a magnetic field on spectral lines.

1896. Stanley Williams' classical paper on the various currents in the apparent surface layers of Jupiter.

1896–9. Schwarzschild's pioneering work on the determination of photographic magnitudes.

1897. Publication of Miss Maury's *Discussion of Stellar Spectra* and of her discovery of the " c-characteristic."

1897. Spectrum of a meteor photographed at Arequipa.

1897. Inauguration of Yerkes Observatory and its 40-inch refractor.

1897. Gill's determination of the sun's distance from heliometric observations of minor planets.

1898. Eros discovered by Witt.

1898. Seeliger's statistical investigations of star distribution.

1898. Lockyer and Fowler find that lines of what are now known as ionized atoms are relatively stronger in flash spectrum than in ordinary solar spectrum.

1899. Keeler's nebular photographs with the Crossley reflector, demonstrating the preponderance of spiral forms.

1899. Scheiner's photograph of spectrum of Andromeda nebula, showing dark lines suggestive of integrated starlight of solar type.

1900. Kapteyn's proposal for more rapid determination of stellar distances photographically, and his specimen results for 248 stars.

1900. Publication of final volume of *Cape Photographic Durchmusterung*.

1900. International Conference in Paris arranges for the co-operation of 58 observatories in determining the sun's distance from observations of minor planet Eros.

1900. Close approach of Eros.

1901. Nova Persei discovered by Anderson : first nova in which the spectrum was observed before maximum.

1901. Miss Cannon's discussion of the spectra of 1,122 southern stars, and her modification of the Harvard classification of spectra.

1901. Kapteyn derives two statistical formulae giving the average distances of groups of stars between given limits of brightness and proper motion.

1901. Campbell determines the velocity of the sun from stellar radial velocities.

1901. Nebula near Nova Persei discovered photographically by Wolf.

1901. Photograph taken later by Perrine shows nebula to be rapidly expanding outwards from nova.

1901. Prosper Henry's use of the coarse grating for the determination of photographic magnitudes.

1901–9. Schwarzschild's determinations of photographic magnitudes for the *Göttingen Aktinometrie*, and his invention of colour-indices.

1903. Adams and Frost draw attention to the remarkably small radial velocities of the B stars.

1903. Slipher begins his pioneering spectroscopic work on the atmospheres of the planets.
1904. Kapteyn's discovery of the two star streams.
1904. Foundation of Mount Wilson Solar Observatory.
1904. Hartmann draws attention to fixed calcium lines in an early type spectroscopic double star.
1904. Jeans suggests annihilation of matter inside a star as a possible source of stellar energy.
1904. Turner studies light-curves of long-period variables by harmonic analysis.
1905. Publication of Einstein's *Special Theory of Relativity*.
1905. Mitchell obtains excellent photographs of the flash spectrum with objective grating spectrograph, which enables him to measure the heights of the various gases.
1905. United States begin the regular radiation of wireless time-signals for benefit of shipping.
1905–7. Hertzsprung's recognition of giant and dwarf stars from his studies of proper motions, and his realization of the significance of Miss Maury's c-characteristic.
1905–9. Cowell's studies of ancient eclipse records and his determination therefrom of the rate of the apparent speeding up of the moon's orbital motion.
1906. Cowell finds that the apparent speeding up of the moon may be explained by gradual increase in the length of the day amounting to 0.005 seconds per century.
1906. Hertzsprung's determination of the apparent diameters of stars from their spectral types and magnitudes.
1906. Kapteyn proposes his plan of "selected areas."
1907. Publication of final volume of *Potsdammer Photometrische Durchmusterung*.
1908. Boss' discovery of the convergent moving cluster in Taurus.
1908. Schwarzschild's "ellipsoidal hypothesis" of star-streaming.
1908. Publication of the *Revised Harvard Photometry*, giving visual magnitudes of 45,792 stars.
1908. Jordan's and Parkhurst's development of the method for determining photo-visual magnitudes with panchromatic plates and filters.
1908. Hertzsprung's first use of coarse grating for determination of effective wavelengths.
1908. Hale's early studies of the magnetic fields round sun-spots.
1908. Evershed's discovery of the radial overflow of gases from sun-spots.
1908. Fowler's detection of titanium oxide in sun-spot spectrum.
1908. Adams finds that the upper layers of the sun's atmosphere are rotating more rapidly than the deeper layers.
1908. Cowell and Crommelin investigate the orbit of Halley's comet, and trace with one exception all of its returns to the sun as far back as 240 B.C.
1908. Fall of the great Siberian meteorite.
1909. First detailed determinations of stellar temperatures (visually) by Nordmann and by Wilsing, Scheiner and Munch.
1909. First photographic discoveries of ordinary novae in Andromeda nebula.

1909. Antoniadi marshals powerful evidence against the geometrical character of the Martian canals.

1910. Hinks' determination of the sun's distance from the observations of Eros in 1900.

1910. Publication of Boss' *Preliminary General Catalogue*, giving proper motions of 6,188 stars.

1910. Boss' determination of the direction of the sun's motion.

1910. Relation between random velocities and spectral types of the stars, discovered independently by Kapteyn and Campbell from studies of radial velocities.

1910. Relation between random velocities and spectral types confirmed by Boss from study of proper motions.

1910. Kapteyn and Frost discover a small systematic recession from the sun of the B stars.

1910. Russell confirms Hertzsprung's division of stars into giants and dwarfs, from direct measures of stellar distances.

1910. Stebbins employs a selenium-cell photometer for the measurement of stellar variability.

1910. Fowler identifies carbon monoxide in tails of comets Daniel and Morehouse, 1907 and 1908.

1910. France and Germany start the radiation of wireless time-signals.

1911. Rutherford's model of the atom.

1911. Campbell's second determination of the direction and speed of the sun's motion, from 1,198 radial velocities.

1911. Halm finds correlation between velocities and masses of the stars, suggesting an approximation to a state of equipartition of energy.

1911. Halm finds evidence of a third star stream, from study of proper motions.

1911. Campbell notices the absence of star-streaming among B stars.

1911. Campbell confirms systematic recession among B stars and finds the effect, though less marked, among stars of other spectral types. He calls it the K-term.

1911. Foundation of American Association of Variable Star Observers.

1911. Slipher determines spectroscopically rotation period of Uranus.

1912. Miss Leavitt's publication of the relation between the periods and magnitudes of 25 Cepheids in the Small Magellanic Cloud.

1912. Publication of Pickering's North Polar Sequence, giving photographic magnitudes of 96 stars from mag. 2 to 21.

1912. Slipher finds spectrum of Pleiades nebula to be a replica of that of the involved stars.

1912. Slipher confirms the close similarity of the spectrum of the Andromeda nebula to that of the sun.

1912. International Conference in Paris for standardization, etc., of wireless time-signals.

1912. Russell's investigation of the theory of eclipsing variables, and of the determination of their orbits, sizes and densities.

1913. Bohr's theory of the atom and of spectral lines.

1913. Fowler's production in the laboratory of the Pickering series of "hydrogen lines," now known to be due to ionized helium.

1913. Russell's and Shapley's determination of the densities of 87 eclipsing variables, showing that the range in absolute magnitude is mainly dependent on density.

1913. The Russell Diagram; and Russell's explanation of the giant and main sequences, and his theory of stellar evolution.

1913. Hertzsprung's preliminary determination of the zero-point of the Cepheid period-luminosity law, and his estimation of the distance of the Small Magellanic Cloud.

1913. St. John confirms the Evershed effect, and shows that at higher levels the flow is reversed, the gases moving radially inwards towards the spots.

1913. Pfund first employs thermo-couple for measurement of stellar radiation.

1913. Meyer and Rosenberg, and Guthnick first use photo-electric cell for study of variable stars.

1913–4. Determination with radio time-signals of the difference in longitude between Paris and Washington.

1913–5. Development by Plummer of his pulsation theory of Cepheids.

1914. Adams and Kohlschutter develop their spectroscopic method for determination of absolute magnitudes and distances of stars.

1914. Eddington shows that the average distances of the two star streams are the same.

1914. Eddington's determination of the direction of the sun's motion from the apparent obliquity of the two star streams.

1914. Slipher's early studies of the radial velocities of spirals.

1914. Coblentz' early measurements of planetary temperatures at the Lick Observatory.

1914–23. Fowler's experimental work on the spectra of ionized and multiply ionized atoms.

1915. Publication of Einstein's *Generalized Theory of Relativity.*

1915. Einstein's theory explains the motion of the orbit of Mercury.

1915. Adams photographs the spectrum of the companion of Sirius, finding it an early type star and thus presumably very small and dense.

1915. Adams and Kapteyn, and independently Campbell, obtain evidence of star streaming from radial velocities.

1915. Bickerton's theory of the meteoric formation of lunar craters.

1915–7. Shapley's investigations of globular clusters show that in general absorption of light in space must be negligible.

1916. van Maanen's measures of the internal motions of spiral nebulae, indicating rapid rotations.

1916. de Sitter's investigations of the astronomical consequences of Einstein's theory.

1916. Eddington's discovery of the importance of radiation pressure in stellar equilibria.

1916. Phillips' harmonic analysis of the light curves of long-period variables, and his discovery of two distinct types of star.

1916. Barnard and Steavenson independently discover the nebulous disc of Nova Persei 1901.

1917. Slipher brings his determinations of the radial velocities of spiral nebulae up to 15, the majority of which are rapidly receding from the earth.

1917. Einstein discovers a method for closing up space.
1917. de Sitter obtains an alternative solution for closing up space, which explains the rapid recession of the spiral nebulae.
1917. Dyson points out the unique opportunity afforded by the total solar eclipse of 1919 for testing Einstein's prediction that light is deflected in a gravitational field.
1917. Eddington concludes that the atoms in the interior of a star must be largely stripped of their electrons.
1917. Publication of Jeans' earlier work on Cosmogony.
1918. Appearance of Nova Aquilae : first spectrum of a nova to be observed in pre-outburst stage.
1918. Shapley greatly improves value of zero-point of period-luminosity law of Cepheids.
1918. Shapley's studies of the distances and distribution of the globular clusters and of the direction of the galactic centre.
1918. Eddington's theoretical investigations of pulsation theory of Cepheids.
1918. Publication of Wright's measurements of the positions and intensities of the nebular lines.
1918. Erection of the 100-inch at Mount Wilson and of the 72-inch at the Dominion Observatory, Victoria, B.C.
1918. First volume of new *Henry Draper Catalogue* published.
1919. Total eclipse of sun : Eddington and Cottingham, and Crommelin and Davidson obtain definite evidence that light is deflected in a gravitational field in accordance with Einstein's prediction.
1919. Evershed's and St. John's early results as to the Einstein shift of spectral lines in the gravitational field of the sun.
1919. Moore and Shane investigate spectroscopically the complex internal motions of the nebulous disc of Nova Aquilae 1918.
1919. Slipher brings the number of his gaseous nebulae with dark lines up to 6.
1920. Saha's theory of ionization in stellar atmospheres.
1920. Eddington's calculation from theory of the diameter of Betelgeuse.
1920. Michelson and Pease with the stellar interferometer succeed in making first direct measurement of a star's diameter. The result for Betelgeuse is in close agreement with that given by Eddington.
1920. Pannekoek's studies of the distances, depths and opacities of the dark nebulae in Taurus, and his deductions as to the density of such clouds and the size of particle composing them.
1921. Lampland and Duncan find evidence for internal motion in Crab nebula, in a direction radially outwards.
1921. Kulik's first expedition to Krasnoyarsk to investigate the Siberian meteorite of 1908.
1921. Steavenson begins his routine observations of the light variation of subsided novae.
1921. Foundation of the French association of variable star observers.
1922. Coblentz and Lampland make more detailed studies at Lowell Observatory of planetary temperatures, and show definitely that Jupiter and Saturn are very cold planets.
1922. Nicholson and Pettit use thermo-couple for measuring variation in total radiation of long-period variable Mira.

1922. Hubble's investigations of galactic nebulae showing a relation between the spectrum of the nebula and that of the involved star; a relation between the extent of the nebulosity and the brightness of the involved star; and the failure of this second relation in the case of planetary nebulae, in which the central stars are much too faint.

1922. First meeting of International Astronomical Union, Rome.

1922. Miss Cannon's final modification of the Harvard spectral classification, as used in the *Henry Draper Catalogue*, officially adopted by I.A.U.

1922. Seares's North Polar Sequence of photographic and photo-visual magnitudes officially adopted by I.A.U.

1922. Confirmation by American and Australian eclipse expeditions of the Einstein deflection of light.

1922. Hertzsprung gives the apparent diameters of 734 stars determined from estimates of their colours.

1923. Aston discovers isotopes: chemically identical atoms having different atomic weights.

1923. H. H. Plaskett determines stellar temperatures from distribution of energy over whole of photographed spectrum.

1923. Sampson simplifies the above method, by studying the photographed spectrum with a self-recording densitometer.

1923. Jeffreys, from theoretical considerations, concludes that the temperatures of Jupiter and Saturn must be excessively low.

1923. Kramer's theory of stellar opacity.

1924. Coblentz and Lampland make detailed measures of temperatures of different parts of Mars' surface at various seasons and various times of the day.

1924. Eddington's discovery of the mass-luminosity law.

1924. Publication of the final volume of the *Henry Draper Catalogue*, which gives spectral types of 225,000 stars.

1924. Milne's theory of the calcium chromosphere.

1924. Hale's invention of the spectrohelioscope.

1924. Hale announces the reversal of the magnetic polarity of the sun-spots in the two hemispheres with the commencement of the new spot cycle. He had observed the reversal originally with the start of the new cycle in 1912; and thus concludes that the phenomenon takes place regularly.

1924. Merrill and Strömberg find all long-period variables to approximate to absolute magnitude 0.

1924. J. S. Plaskett finds that the interstellar calcium clouds give in general a consistent value for the sun's motion, as though they were stationary in space.

1924. Wright's photographs with colour filters indicate a fairly extensive Martian atmosphere.

1924. R. H. Fowler and Milne investigate, by means of modification of Saha's theory, the pressures in stellar atmospheres.

1925. Change in the time of commencement of the astronomical day: put *back* 12 hours from mid-day to midnight.

1925. Hubble determines the distance of the Andromeda nebula from Cepheids.

1925. Lundmark determines the distance of the Andromeda nebula from novae.

1925. Lundmark finds linear relation between the distances of the spirals, as judged by their apparent sizes, and their radial velocities, in agreement with de Sitter's relativistic theory of their recession.

1925. Adams measures the Einstein shift in the spectrum of the companion of Sirius, which shows this star to have an almost incredibly high density. This is the first definite confirmation of Einstein's third prediction.

1925. Strömberg's discovery of the asymetry of the motions of high velocity bodies.

1925. Nova Pictoris discovered by Watson; studied in great detail at the Cape during its unusually slow rise to maximum.

1925. Miss Payne finds the majority of stars to be essentially similar in chemical composition.

1925. Second meeting of the I.A.U., Cambridge.

1925. First Shortt Free Pendulum Clock installed at Greenwich.

1926. Lindblad's general theory of galactic rotation.

1926. Bjerknes' theory of sun-spots and the sun-spot cycle.

1926. Brown concludes that certain irregularities in the moon's motion are due to variations in the length of the day produced by expansions and contractions of the earth's crust.

1927. Bowen identifies theoretically the chief nebulium lines with those of singly and doubly ionized atoms of oxygen and nitrogen.

1927. Oort's detection and measurement of the rotation of the galaxy.

1927. Slipher brings his determinations of the radial velocities of spiral nebulae up to 40; almost all rapidly receding.

1927. Lemaître's original paper on the expanding universe theory.

1927. Zanstra suggests that the discrepancy between the brightness of a planetary nebula and that of its central star must be due to the light of the star being mainly in the extreme ultra-violet, and should give a measure of the star's temperature.

1927. Harvard southern station moved to Bloemfontein.

1927. Kulik's second expedition succeeds in penetrating to site of the 1908 meteorite fall in Siberia.

1928. Moore and Menzel determine spectroscopically rotation period of Neptune.

1928. Publication of Jeans' *Astronomy and Cosmogony.*

1928. First large optical glass disc cast and annealed in America, by the Bureau of Standards, for the Perkins Observatory, Ohio.

1928. Third meeting of I.A.U., Leiden.

1929. Hubble determines the velocity-distance relation for the spiral nebulae and finds that their radial velocities increase at the rate of about 500 kilometres per second per 3·3 million light years.

1929. Eddington's early work on the theory of the expanding universe.

1930. Trumpler finds evidence for considerable absorption of light in space in the galactic plane.

1930. Lyot from summit of Pic du Midi observes the bright lines of coronal spectrum and detects the polarization of coronal light without an eclipse.

1930. Discovery of Pluto at Flagstaff by Tombaugh.

1930. The pioneering work of Shajn, Struve and Elvey on the determination of stellar rotation speeds from the widening and detailed " profiles " of their spectral lines.

1930. Independent determinations of Zanstra and Berman of the extremely high temperatures of the central stars of planetary nebulae.

1930. Milne's theory of white dwarfs and of the origin of novae.

1931. Very close approach of Eros, extensively observed for re-determination of sun's distance.

1931. Lyot from Pic du Midi obtains direct photographs of corona without an eclipse.

1931. Spencer Jones' investigations of the expansion of Nova Pictoris during its rise to maximum in 1925.

1931. Humason's early results with the 100-inch in determining radial velocities of fainter and more distant spirals. These also fit in satisfactorily with Hubble's velocity-distance relation.

1931–3. Harvard meteor-expedition to Arizona shows that large majority of sporadic meteors come from outside the solar system.

1932. Milne's theory of the observed expansion of the universe.

1932. Wildt identifies some of the unknown bands in the spectra of the major planets with bands of ammonia and methane.

1932. Adams and Dunham discover carbon dioxide in considerable amounts in the atmosphere of Venus.

1932. Humason obtains the first photographs of spectra of novae in the Andromeda nebula, finding them similar to those of ordinary galactic novae.

1932. Hubble discovers 140 globular clusters forming a system surrounding the Andromeda nebula.

1932. Fourth meeting of the I.A.U., Harvard.

1932. Cockroft and Walton at Cambridge succeed in the artificial transmutation of a few of the lighter atoms into heavier atoms.

1933. Carroll's further development of the method for determining stellar rotations from the profiles of spectral lines.

1933. Strong's development of the method for aluminizing the surfaces of telescope mirrors ; and the aluminization of the Crossley reflector.

1933. Dunham with high-dispersion spectrograms confirms the identification of methane and ammonia in Jupiter and Saturn.

1933. Stebbins, with photo-electric photometer, determines reddening of globular clusters near Milky Way and amount of galactic absorption of light.

1934. The pouring by the Corning Glass Company in New England of two 200-inch discs of optical glass for the 200-inch telescope to be ultimately erected on Mount Palomar.

1934. Hubble determines the galactic absorption of light from the distribution of the spiral nebulae.

1934. Shapley's studies of photographs of extra-galactic nebulae with self-recording densitometer, show them to be much more extensive systems than was previously supposed.

1934. Vorontsov-Velyaminov's investigations of distances of planetary nebulae indicate that many of their central stars have densities comparable with white dwarfs.

1934. Baade's and Zwicky's investigations and theory of super-novae, and their suggested campaign for the discovery of these objects.

1934. Great increase in range and sensitivity of near-infra-red photographic plates. Their use by Merrill for survey of infra-red stellar spectra between wavelengths 7000 and 9000.

1935. Russell's general theory of planetary atmospheres: major planets, intermediate or terrestrial planets, and minor planets; and his explanation of the red colour of the Martian surface.

1935. Kuiper's discovery of a star about 600 times denser than the companion of Sirius.

1935. Fifth meeting of I.A.U., in Paris.

1935. The aluminizing of the 100-inch mirror at Mount Wilson.

1935. The retirement of Phillips from directorship of the Jupiter Section of British Astronomical Association, having published 19 memoirs dealing with 33 years' observations of the planet.

1936. Discovery of a super-nova in the Virgo super-galaxy as result of a seven-year search by Baade, Zwicky and Hubble.

1936. Its spectrum photographed by Humason shows bright lines much more broadened than in ordinary novae, indicating an expansion rate of 4,000 miles a second.

1936. Johnson re-discusses spectrum of super-nova in Centaurus (1895)—the only other super-nova whose spectrum had been photographed—and finds a similar enormous broadening of the bright lines.

1937. Discovery of three super-novae as result of new systematic search by Baade and Zwicky with Schmidt camera on Mount Palomar (future site of 200-inch telescope). One reaches mag. 9, and Humason photographs typical super-nova spectrum.

1937. Stevens' aeroplane photographs at 25,000 feet of eclipsed sun, suggest that coronal streamers are enveloped in an outer globular corona.

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